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BEHAVIOR AND THE CENTRAL NERVOUS SYSTEM

BY ALBERT P. WEISS

Ohio State University

In reviewing the similarities and differences that exist between the introspectionists and the behaviorists, Professor H. C. Warren[1] concludes that "If we lay aside our traditional prejudices and the bias attaching to the terms 'psychology' and 'behavior,' we cannot but admit the close relation between the events studied by the two groups. Both 'conscious experiences' and manifestations of 'behavior' are phenomena of *organic creatures*. And further, except in the lowest creatures, they are intimately associated with the operations of the *nervous system*. . . . A balanced view of the field of psychology, then, would seem to make its central feature the *specific activity of the nervous system*" (p. 249).

As a behaviorist, the writer[2] doubts whether a series of fundamental conceptions acceptable to both introspectionist and behaviorist can be developed. However the challenge which Warren implies is so obviously appropriate under the controversial conditions that exist, that the compromise on fundamental principles which he has formulated should

[1] Warren, H. C., 'Psychology and the Central Nervous System,' PSYCHOL. REV., 28, 1921, 249–269.

[2] In this article the writer speaks only for himself. Behaviorism does not mean the same thing for all who use the term. In the historical development of behaviorism the first attempts were directed against the typical introspectionist experiment. For this reason behavioristic literature is still largely concerned with defending itself against the traditional method of formulating a psychological problem, instead of setting its own problems in its own way.

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receive the sympathetic and critical consideration of both groups of investigators. Before taking up Warren's points in detail it will perhaps simplify the discussion to give in outline form what the writer regards as the various stages in a thoroughgoing behavioristic investigation.

The Behavior Problem in Outline

Definition.—Behaviorism is the science that studies the origin and development of those bodily movements (reactions) of the organism which establish its social status. The three parts of the problem are: (1) The stimulating condition, (2) sensory-cerebro-motor condition, (3) the overt reaction.

1. *The Stimulating Condition.*—This may be as simple as that of a loud sound, or as complex as that of the written record of an event in history. The stimulating condition presents two aspects, each one of which has its own categories of classification:

(*a*) The *biophysical* aspect, described in such units as light waves, sound waves, motions, directions, areas, durations, temperatures, vapors, pressures, liquids, etc.

(*b*) The *individual-social* aspect, described and classified according to the degree of similarity that it produces in the overt actions of organisms. The categories that describe the individual-social aspect of the stimulating condition group the stimuli according to their similarities as signals[1] for other organisms. Similar signals are not usually physically commensurate nor need they be similar signals for all organisms, or even for the same organism at different times. Thus the visual stimulus of the written words, 'next Tuesday,' 'mardi prochain,' 'nächsten Dienstag,' to use Warren's illustration, may be identical as signals for some particular

[1] Some confusion is likely to result unless the terms 'stimulus' and 'signal' are carefully discriminated. The writer tries to use the word stimulus only in the biophysical sense, reserving the term signal for the individual-social aspect. If this differentiation is maintained, equal stimuli may be unequal signals; unequal stimuli may be equal signals. The term signal indicates that a stimulus is being considered with reference to the sensory-cerebro-motor modifications that have occurred in some organism. The term stimulus itself has no such reference.

individual and for other persons having the same social status. They would still maintain their *individual-social* identity even if converted into an entirely different physical category, as for instance into the sound waves of the oral words instead of the visual printed words. On the other hand for a laborer, representing a different social status, the three pairs of words would be different kinds of signals because they released different responses.

2. *The Sensory-cerebro-motor Conditions.*—These are to be regarded in the strictly neurological sense as including a study of the receptor and effector mechanism, the so-called cerebral integrating mechanism, and the incipient or implicit motor and glandular phenomena preceding or accompanying the final explicit or overt reactions. From a study of these conditions, the behaviorist learns the limitations that the structure and function of the sensory-cerebro-motor system imposes upon the types of overt reactions that may or may not be formed by the organism. The behaviorist would not maintain, however, that a given reaction, say that of finding a certain word in the dictionary, had the same sensory-cerebro-motor antecedents, either for two different persons or for the same person at two different times.

3. *The Overt Reaction.*—The overt reaction is the group of movements that directly or indirectly establish the social status of the organism. Like the stimulating condition, the overt reaction presents two aspects:

(*a*) The *biophysical* aspect, to be described in terms of motion, energy, extent, anatomical elements, physiological variables such as fatigue, rest, practice, inhibition, nutrition, heredity, etc.

(*b*) The *individual-social* aspect, in which the reaction is regarded as having a social as distinct from a biological history and which reaction may be classified into anthropological, ethnic, or social categories.

The overt reactions of one organism may be signals for other organisms and as such present similarities and uniformities that are incommensurate with the biophysical characteristics of the reaction. Thus the overt action of

signing my own name may be biophysically identical on two occasions but if in the one case the name is signed to a check and in the other to a note, the two actions are entirely dissimilar from the social standpoint because one action stamps me as a creditor, the other a debtor. These socially are two different categories.

The Social Status.—The concept 'social status' that occurs in the definition of behaviorism is to be thought of as the product of two factors:

1. The *specific factor* (*S*) according to which the individual is assigned as a participant in the educational, political, vocational, and recreational activities of the community. Each of these activities is divided and subdivided into specialized categories. Thus the educational activities may be characterized by the many types of educational institutions, as elementary school, high school, university, business college, or into the different curricular classifications as history, mathematics, botany, chemistry, psychology, language, medicine, etc. The political activities would include the types of units represented by the various governmental functions. The vocational activities may be divided into the many trades and numerous professions; the recreational activities, into the many sports, clubs, art, music, and other forms of recreation.

No scientific attempt has yet been made to classify the overt reactions into specific behavior categories. Such classifications as exist are largely based on economic, geographic, and traditional circumstances which for the most part do not conform to the sensory-cerebro-motor differentiations which exist, say, between the activity of bricklaying and that of banking.

2. The *efficiency factor* (*E*) which indicates the position of the individual with respect to other individuals having the same specific factors. Thus of two lawyers, one may be *more able* as judged by his colleagues. In so far as the efficiency factor is evaluated under present social conditions, it is indicated in various ways: rank, opinion, school grade, percentile, expert, journeyman, novice, etc. An attempt at

this time to determine the sensory-cerebro-motor conditions underlying the efficiency factor is perhaps premature though this of course is one of the problems of the behaviorist.

In terms of these two factors the social status of the individual may be represented as follows: Let S_1, S_2, S_3, . . . S_n represent the specific forms of an individual's repertory of overt reactions, and E_1, E_2, E_3, . . . E_n his rank in each of these activities. His social status may then be represented by S_nE_n. Concretely a given individual's social status may be expressed somewhat as follows: High-school graduate (S_1) of average grade (E_1); republican political affiliations (S_2) but not an active worker (E_2); corporation lawyer (S_3) by profession, ranking among the best ten (E_3) lawyers in the community; plays good (E_4) game of golf (S_4) but is poor (E_5) at poker (S_5) and an average (E_6) after-dinner speaker (S_6); very active (E_7) in the Methodist church (S_7), etc. The subnumerals are not meant to represent rank; they merely indicate which S and E factors go together. The writer believes that a very important part of the behaviorist's problem is that of establishing behavior categories which will displace the traditional methods of representing the social status of the individual.[1] The traditional psychological classifications of individuals into visual types, auditory types, kinesthetic types, etc., are useless as individual-social categories because they do not differentiate human beings into classes which approximate the relative achievements of the individual in society.[2]

[1] Without at this time attempting to establish the fact, the writer suggests that the reaction against introspection and the growing interest in behaviorism is in part due to the fact that the older psychological systems ignored this individual-social aspect of the reaction and the stimulating condition. To classify human behavior on the plan outlined is not merely another name for classifying according to the mental states that an individual may have.

[2] Much of the criticism that has been directed against mental testing arises from the failure to see that mental tests are actually *social* tests; that the mental test score actually gives the individual's social status in the specific activity that is being tested. Mental age, fundamentally, means social age. The criticism that 'mental testers do not know what they are testing' merely means that no scientific classification has been developed for normal adult individuals which is based on the overt reactions characteristic of a given group. In school subjects and in the abnormal forms of behavior (psycho-neuroses) the classifications are more scientific and hence

Referring again to the outline of the behaviorist's problem, it is to be understood that the different parts may receive varying degrees of emphasis depending upon the nature of the problem and the interest of the investigator. The individual-social aspects (1*b* and 3*b*), seem the ones which have been neglected most, but these are just the ones which Warren has omitted from his list of fundamental principles, unless they are implied under his 'subjective aspect,' 'personality,' 'experience,' etc. Warren's suggestion that behaviorism makes its *central* feature the specific activity of the nervous system would limit the study of human behavior to (2) and perhaps (3*a*) of the outline. For the behaviorist the nervous system is only a part, a very important part to be sure, of the whole problem. The individual-social aspects (1*b* and 3*b*) are equally important and at this time perhaps more important because they have been neglected so long. In addition to studying the neurogram of a given act, the behaviorist also wishes to know how this act changes the social status, either of the subject, or of those individuals for whom this act is a stimulus or signal of some sort.

Until it is clearly recognized that stimuli and reactions can be classified in two different ways, biophysically and socially, the problem of human behavior, from whatever standpoint it may be considered, will present a mass of contradictions.

Leaving the formulation of the behaviorist's problem as the writer sees it, we may pass to a consideration of the topics discussed by Professor Warren.

The Double-Aspect View.—Taking up the theoretical principles underlying the 'double-aspect' interpretation of behavior, Warren states that "According to the two-aspect view the 'neural correlate' (of consciousness) is not a cor-

the tests are more satisfactory in these fields. The apathy of the mental testers toward the problem of 'what intelligence really is' only means that there are many S_nE_n combinations which, from the social standpoint, are equally valuable, but which from the sensory-cerebro-motor standpoint are entirely dissimilar. In other words, the difficulty with the definition of intelligence means that at present it is impossible to separate the social from the neural factor in the analysis of the overt reaction.

relate at all; it is the *same set of events* observed in a different manner. The book stimuli excite certain optic nerve impulses which are conveyed to the brain; an integrative process occurs in the cortical centers. This cortex is part of my body. I am somehow identified with this particular brain in a sense that I am not identified with the brains of other organisms. . . . That the events which are observed (so far as they *are* observed) by the neurologist in the form of neural impulses, integrations, etc., are the same events which I, the personality of this organism, observe in the form of perceptions and other experiences? That there is actually but one set of primary occurrences concerned, which may be observed either *objectively* by means of apparatus registering the nerve impulses, or *subjectively*, by living these events—by experiencing them as part of myself?" (p. 255).

The writer confesses that he cannot see why such a sharp distinction should be drawn between what occurs in his body and what occurs within the body of another. When it is said that the events which are observed by the neurologist in the form of neural impulses, integrations, etc., are the same events which I, the personality of the organism, observe in the form of perceptions and other experiences, just what does this mean?

If it were possible for me to observe my own neural processes in the same sense that the neurologist observes them, visually perhaps, besides seeing what the neurologist sees, I should also act out in my own reaction mechanism the actual nervous processes which I see ramifying through my own nervous system. Imagine an apparatus of an X-ray type which makes visible the nervous processes that are actually taking place in my own nervous system. Think of this apparatus so arranged that it has two eye-pieces; one for the neurologist, one for myself. Suppose we were actually observing the nervous processes which occurred in a pain (withdrawing) reflex. The neurologist sees the sensory-cerebro-motor processes. So do I, but in addition *my* muscles in which the nervous processes terminate, contract. The corresponding muscles of the neurologist do not contract. If now both of us write up independently a full report of

our observations, I can include something (the kinesthetic activity) which the neurologist can not include because his own corresponding muscles have not contracted. Even if the neurologist *saw* all my kinesthetic processes, and saw the contractions of all the muscle fibers, the totality of the biophysical processes in my own body during the period of observation are different from the totality of the biophysical processes in the neurologist. This is to be expected since two separate protoplasmic systems are being subjected to different biophysical stimulating conditions and it is to be expected that the end results, the verbal report of our observations, would be different, because the verbal report is only motor activity of the speech muscles and like all other motor phenomena dependent on the biophysical stimulating conditions which preceded. Since my biophysical configuration differs from that of the neurologist it is to be expected that my report will differ from his, and that it will contain different words.

If the preceding experiment is simplified to approach the conditions in which only the neurologist observes (visually) my nervous processes and reactions while I am eating an orange, the neurologist sees: neural impulses of varying intensities localized in specific neurons; salivary secretion of greater or less amount; movements of the lips, tongue; movements of the stomach, etc. I myself do not see these things, simply because my own eyes are not stimulated by my neural processes or by my muscle contractions. Why should not my reactions (experience) differ from those of the neurologist as measured by the totality of the biophysical changes produced in both of us during the period in question? Part of the neural processes in me may interfere with each other because they go to antagonistic muscles so that I spit out, or stop eating. A part of the neural processes may release the speech reaction, "This orange is *unpleasantly* sour." None of these things, however, occur in the body of the neurologist who is watching me. Is it any wonder that I am reacting differently (having different experiences) from the neurologist? Further, the neurologist can never

react as I react (have my experiences), because my nervous system, or my protoplasmic system is not continuous with his. My sour taste can never be his sour taste for the very simple reason that when my taste organs are stimulated by acid, the nervous impulses that are generated do not get into his nervous system. Is it necessary to describe this purely anatomical and physiological circumstance as 'personal' 'subjective'?

For the behaviorist, then, the 'subjective aspect' of the 'primary occurrences concerned' only means that the sensory-cerebro-motor processes of the subject are not the same as the sensory-cerebro-motor processes of the person who is observing him, and that they can never be the same because the two biophysical systems are not anatomically connected. This fact is so self-evident that there is no need for lifting it out as an aspect or as something 'subjective,' especially since this term is perhaps the most abused word in the English language.

For the behaviorist, then, the double-aspect view is unnecessary. Using a strictly monistic basis he may describe all the stages of his problem by the properties of protoplasm if he is biologically trained; or by the configurations of electrons and protons, if he is following the recent work in physical chemistry.

Thinking.—Professor Warren next considers memory, imagery, and thought. Interpreting the behavioristic position he states, "When I think, the process consists in very faint vocal expressions and vocal attitudes. So-called imagery consists in minute responses which reproduce faint stimuli and thereby reinstate minute sensations similar to the previous sensations, though far lesser in degree. There is surely a logical fallacy in this assumption. Granting that the central nerve activity is always followed at once by a motor impulse and response of some sort, it does not follow that such responses are the *essential feature* of the perceptual, discriminative, and imaging processes. They may be merely by-products" (p. 258).

For the writer the conception of 'thought' as outlined by

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Warren is not inclusive enough. Suppose for instance two men are asked to design a piece of apparatus for an experiment. The problem is presented to both men in the same way. They go into separate rooms and after a few hours submit a design of the apparatus. Inspection shows that the designs are practically identical. Now did these men have the same thoughts? It depends, of course, on how we define thought. A non-psychological scientist would perhaps express surprise at the similarity and uniformity of their thought processes, basing his conclusion on the fact that the *product* of their efforts, the designs, were alike. Thought defined in this sense emphasizes its practical or social value.

But let us ask, did the two men have the same images, in the same order, in the same intensity, for equal durations? Or speaking behavioristically, were all their neural processes, all their subvocal and other implicit reactions, all their secretions, of the same order, the same intensity, of the same duration? Surely not. If thought is defined in terms of these central processes and implicit reactions instead of the social value of the product, then their thoughts were *not* alike. Again, if we consider the interval between the stimulating conditions and the final reactions (completed designs), the sensory-cerebro-motor processes in each of the men, while not alike, were undoubtedly the essential antecedents of the act of designing. Shall we say that there was similarity in that the neural processes were restricted to some particular cortical level? Shall we then define thinking as neural activity at a given level, or as 'essentially a central nervous operation' as Warren does? Evidently thinking may be defined in at least three ways: in terms of the product, the incipient reactions (imagery), and the central nervous operation. Is it not better to eliminate the word 'thought' altogether and substitute for it a description of the individual-social conditions which everyone will agree represent a very important problem in human behavior by whatever name these conditions may be called. Such a description of the

designing activity used as an illustration would be the following:

1. A stimulus of the problem type. Example, What is the square root of 39?
2. An interval of longer or shorter duration. During this interval central nervous operations of a specific order and many overt and implicit reactions may occur, as for instance, consulting a Table of square roots, etc.
3. An overt response. The presentation of the number 6.245, the conventional (individual-social) solution or partial solution to the problem stimulus.

What is ordinarily called scientific thinking involves all three of these factors. The investigator may be interested particularly in one of the stages, say stage 2, but to say that a complete description of 2 is the equivalent of 1 and 3 is committing what Warren calls the thermometer fallacy[1] with a vengeance.

Such a differentiation of the problem presents a definite experimental program without introducing a terminology and misconceptions that were developed during a time when an animistic conception described human behavior as a product of imponderable, immaterial forces which natural science has long ago abandoned.

Equivalence in Central Nervous Processes.—Passing on to the fact that stimuli may be equivalent even though physically incommensurate, Warren calls attention to the following illustration: "There are instances of pairs or groups of responses which are quite different yet which are obviously equivalent, where the 'equivalence' can be traced to the central nervous operations. I write the word 'Next Tuesday' very fine, using only the muscles of the finger joints with slight wrist movements; then I write the same words in eight-inch letters on the black-board with fingers and wrist perfectly rigid, using only the muscles of the elbow and shoulder. . . . Or, I hammer the words on a typewriter, click them off on a telegraph key, set them up in type, scrawl

[1] The thermometer fallacy which Warren mentions is best given by his quotation, "We are not warranted in identifying the central phenomena of discrimination and thought with the response phenomena which they produce" (p. 258).

them stenographically. . . . Or, I utter the words 'mardi prochain,' 'nächsten Dienstag'; these are in a sense equivalent to all the former responses. The psychological relation between these various responses is not brought out by a study of behavior. It is certainly dependent in a measure upon the stimuli, upon the general situation which led to the responses. But the basis of the 'equivalence' between such different responses seems to lie primarily in the central nervous system" (p. 260).

From this quotation Warren seems to place a limitation on the term behavior which is not shared by the writer. When he states that "The psychological relation between these various responses is not brought out by a study of behavior" the writer wonders what the term 'psychological' means. If Warren means by the term psychological what the writer has called the individual-social aspect, then behaviorism offers the *only* solution of the problem. The basis of the 'equivalence' is not in the nervous system of the reactor but in the nervous systems of *other* individuals of the same social status.

After pointing out the neural differences between the various forms of responses given in the quotation, Warren asks, "How can motor currents be sent out producing sets of behavior which are physically incommensurate with one another (writing, vocalizing in various languages) and yet have practically the same 'significance'" (p. 261).

From the behavioristic point of view the only equivalence that exists lies in the fact that all the reactions are *equivalent stimuli* (equivalent signals) for all individuals whose sensory-cerebro-motor mechanism has been modified in a certain way. In other words the reactions are *socially* equally significant. They are certainly not equivalent in any neurological sense, either afferent, central, or efferent. No amount of neurological insight will reveal the fact that all these reactions are equivalent signals for all individuals of a certain social status, or that they are different signals for individuals of a different social status. This is a strictly behavior problem and involves an ethnic, anthropological, or social analysis of

the reactions, and of the stimulus conditions that originally released these reactions.

The Use of Neural Data in Psychology.—The writer in general would agree with Warren. If introspection is regarded as merely a form of verbal behavior which directly or indirectly localizes obscure receptors; makes it possible to infer the nature of the nerve impulse; reveals incipient effector phenomena; or makes it possible to plot, as it were, the sensory-cerebro-motor constants as modified by past function; it is just as valuable for the behaviorist as for the introspectionist. In other words, in so far as the introspective reactions are regarded as inherited and acquired precision instruments for the indirect measurement of such neural conditions as excitation, retention, conduction, metabolic variation, summation, distribution, modification[1] the behaviorist will use them and thankfully express his indebtedness to the introspectionist psychologist who has undoubtedly contributed much toward the development and refinement of this indirect method of investigating sensory-cerebro-motor phenomena. But this type of problem is not of any particular interest to the behaviorist unless it is brought into relationship with those overt reactions which have a social significance. The behaviorist believes that, for himself, studying these processes '*as such*' is no more fruitful[2] in developing laws of human behavior than studying consciousness *as such* has been fruitful in developing 'laws of mind or consciousness.' To the organism the environment presents itself as a chaotic mass of stimulating forces; in the same way, the effector system presents a chaotic mass of contractile and secretory elements. What relations can be established between the stimulating situation and the reaction system will of course depend on the properties of the nervous system and the laws of neural function. But the behaviorist is not a neurologist. His problem is to investigate *all* the conditions that determine the social status of the individual in society.

[1] This list is taken from Warren, H. C., 'Human Psychology,' 1919, Table VII., p. 144.

[2] The study of these neural conditions *as such* is straight neurology, not behaviorism in the sense in which the writer understands the term.

Summary

For the writer behaviorism is the science that studies the origin and development of those bodily movements of the organism which establish its social status.

The outline of a typical behavior problem includes: (1) The stimulating situation, resolved into (*a*) the biophysical aspect, and (*b*) the individual-social aspect. (2) The sensory-cerebro-motor conditions and incipient or implicit motor or glandular phenomena preceding or accompanying the final overt reaction. (3) The overt reaction, resolved into (*a*) the biophysical aspect, and (*b*) the individual-social aspect.

The social status of the organism is described by S_nE_n, where S represents the social categories into which its educational, political, vocational, and recreational activities may be divided, and E the rank of the organism in these activities.

Professor Warren's conception of behaviorism would limit it to a study of the responsive phase of the sensory-cerebro-motor system. This is too narrow. In addition to the neural conditions, behaviorism studies the stimulating conditions from the standpoint of the social categories into which they may be classified, and studies responses from the standpoint of their significance as stimuli (signals) in establishing the social status of the organism.

The double-aspect view of the mind-body relation as developed by Warren, seems for the behaviorist to be merely a recognition of the fact that the sensory-cerebro-motor systems of organisms are not anatomically continuous between the separate organisms. The behaviorist sees no need for introducing any modification into the essentially monistic principles underlying either biology or physics.

Memory, imagery, thinking, for the behaviorist are parts of behavior categories that include an individual-social aspect which is not recognized in the traditional psychological classifications. Thus one such category which includes what is usually regarded as 'thinking' may be described as: (*a*) A stimulus situation of the problem type, followed by (*b*) an interval during which implicit and explicit reactions occur, and finally (*c*) an overt response which is the conventional

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solution to the problem stimulus. Whether stage (*b*) or stage (*c*) or stages (*b*) plus (*c*) be described as thinking is quite immaterial. The behaviorist maintains merely that all three stages are very significant factors in establishing the individual's social status.

It is not necessary to assume a specific central factor, as Professor Warren does, to account for the responses which are physically incommensurate with each other (writing as compared with vocalizing) but which yet have the same 'significance.' The 'equivalence' of such responses is due to the fact that they are socially equivalent stimuli (signals) for other organisms having the same social status.

The behaviorist is not opposed to using the introspective method in investigating the properties of the sensory-cerebromotor system. However, the behaviorist does not study such processes as conduction, retention, summation, etc., *as such*. He studies them as important conditions in the development of those overt reactions that establish the individual's social status.

Finally the writer does not believe that the behaviorist and the introspectionist can ever get together. They are working on two different problems. The behaviorist is more concerned with the movements of *other organisms* than he is with his own. Whether the neural processes go over one neuron or another, or through one center rather than another, is quite irrelevant except in so far as the nature of the neural mechanism limits those overt reactions which, when expressed in abstract form, represent the civilization or culture of some social unit, whether this be a nation or a colony of ants. Such similarity as seems to exist between the introspectionist and the behaviorist is illusory, largely due in the writer's opinion to the fact that the contemporaneous behaviorists in psychology have been recruited from the ranks of the introspectionists, and to the fact that the behaviorists have been devoting most of their time to meeting the criticisms of the introspectionists, rather than formulating their own problems in their own way. As time goes on the gap between them will grow wider. If there were no fundamental differences, no differentiation would ever have taken place.

HOW ARE OUR INSTINCTS ACQUIRED?

BY ZING YANG KUO

University of California

In an article entitled 'Giving up Instincts in Psychology'[1] I attempted to repudiate the current conception of instinct and suggested a new hypothesis as a basis for an experimental genetic psychology. Since the publication of my paper there has been considerable discussion concerning my position.[2] The discussion seems to have concentrated more on my constructive view, namely, the reaction-unit hypothesis, than on the repudiation of instincts. While it seems encouraging to see that the anti-instinct movement has not been altogether unfavorably received, there is danger of misunderstanding the motive of the movement and my own position in particular. For example, Mr. Geiger, in quoting my article, has substituted the term '*re*action system' for 'action system.'[3] Such a substitution and, in fact, the whole argument of his paper, seem to indicate insufficient understanding of my central position. I feel it necessary, therefore, to restate the reaction-unit hypothesis more carefully and to give a tentative analysis of the process

[1] *J. of Phil.*, 1921, **18**, 645–664.

[2] Tolman, E. C., 'Can Instincts be Given up in Psychology?' *J. of Abnormal and Social Psychol.*, 1922; Geiger, J. R., 'Must We Give up Instincts in Psychology?' *J. of Phil.*, 1922, **19**, 94–98; Dunlap, K., 'The Identity of Instinct and Habit,' *J. of Phil.*, 1922, **19**, 85–94; and Hunter, W. S., 'Modification of Instincts,' *J. of Phil.*, 1922, **19**, 98–101.

[3] *Op. cit.*, p. 97. By action system I mean the gross bodily architecture of the organism in general and the structure of sense organs and response organs. I do not by any means refer to the preëstablished nervous arrangements, which have long been regarded by the instinct psychologists as the physiological basis of instincts. The number and types of activities which a given organism can perform are largely determined and conditioned by its physiological structure, the action system. It is obvious that no organism can fly without wings, nor can it use tools without hands. This is no assumption of the existence of instincts in any sense of the term as it is commonly understood by the instinct psychologists. On the other hand, by *re*action system I mean organized habitual response which has been integrated directly or indirectly from the units of reaction.

of the acquisition of habits or 'instincts', making my constructive view more adequate and complete. I must, however, ask the indulgence of the reader while I make a few more impolite remarks about the instinct psychologists, before I state my positive view of the matter. For unless the harm done by the conception of instincts to every field of psychology, especially that of genetic psychology, is fully exposed, the reader will be apt to question the usefulness of the whole anti-instinct movement.

Why Should Instincts be Given up in Psychology?

1. *Instinct as a Finished Psychology.*—If we trace the origin of the fiction of instinct we find that it is a conception of primitive man. Being unable to account for the seemingly miraculous nature of animal behavior primitive man attributed this to the Divinity; namely, that the Divinity endowed the animals with certain powers which enabled them to act as they did. Whether primitive man called such divine powers instincts or not, the conception of instinct in modern psychology is essentially the same. Of course, we have considered our own view of instinct as far more advanced and more scientific than the conception of primitive man, for we have substituted 'inheritance' for the 'Divinity' (and the psychologists as well as biologists often think that the inheritance of instincts and emotions and mental traits is indisputable since the Mendelian law has been so well established by the inheritance of *Sweet Peas!*). But how much has this 'scientific' conception of instinct done in the way of explaining behavior, besides taking the place of the Divinity? Nothing, it simply explains away all the problems in behavior which need to be explained: when you are asked why a man turns to the right or to the left you say it is an instinct, and you have answered the whole question! In short, instinct is a 'finished' psychology. It is a stumbling-block in the way of experimental genetic psychology.

We may write volumes on instincts, define and redefine them, classify and reclassify them as we please. But this is all we can do about them. When we come to the actual

facts of the instincts, that is, when we are required to prove their actual existence, we are forced to confess that we cannot find any instincts in man (as there has been no such evidence) not because instincts do not exist, but because they have been so overlaid or modified by habits that it is almost impossible to study them in their pure form, or because they have not reached the period of their appearance. Nay, there is such a thing as the waning of instincts, and when a given instinct is not found to exist it may also be assumed that it has waned. Are not these theories of modifications, waning and periodic appearance of instincts, altogether quibbles? Are they not merely devices made by the instinct psychologists to evade their difficulties in the face of facts? And how many psychologists are there who can defend instincts without making such quibbles? And note, instincts so conceived are bound to be unexperimentable; they are made transcendental by such conceptions, to use a philosophic expression.

Whether all the opponents of instincts will agree or not, I believe our chief motive for denying instincts is to rescue psychology from armchair speculation; we mean to remove this stumbling-block from genetic psychology. We make this as our first attempt towards a genetic psychology which is to be based purely on experimentation. All the writings on instincts are but what Watson has characterized as 'armchair varieties' (and, of course, my own articles on this subject are no exception); these are signs of our disinclination to study the development of behavior by slow scientific procedure, they are signs of our lack of patience in doing painstaking experimental investigation. There are some who think that our aim in rejecting the instincts is to have a better usage of terminology in psychology. But this is not the target at which we are aiming. If it were only a matter of definition and terminology, it would be foolhardy for us to demand so seriously the abolition of the whole conception of instinct. Indeed, if our attempt failed to challenge any experimental investigation on the developmental phase of behavior, if it does not succeed in gradually establishing a

genuine genetic psychology, if the whole anti-instinct movement were to be ended merely in arguments and counter arguments with the instinct psychologists, we must agree with Professor Hunter that the prospects of the movement are not very encouraging,[1] and we should be committing the same sin of making psychology the subject of pure speculation of which we have accused the instinct psychologists.[2]

2. *Further Criticism of Experimental Studies on Instincts.*— We are here brought face to face with the seeming difficulty of handling the so-called experimental evidences of instincts in animals. In an earlier paper we pointed out that although an organism may be able to accomplish certain end results without previous education, the constituent activities which are combined to produce such a result are acquired.[3] But this is not sufficient reason, according to Professor Hunter, for discounting the instincts. "In order to disprove the existence of delayed instincts," he says, "it is not sufficient to indicate that elements of the response have been exercised before. It is necessary to account for the somewhat sudden grouping of the elements into a significantly new response under the conditions where the influence of habit formation is experimentally controlled and negligible." [4] Here we must

[1] *Op. cit.*, p. 100.

[2] It will be well to note that we are not denying the existence of instincts in man alone; animals possess no more instincts than the human organism. During the past two years a group of writers (for example J. R. Kantor, L. L. Bernard, C. E. Ayres, and E. Faris) have attempted to repudiate human instincts of the mystical sort as represented by McDougall, but they still insist that instincts do exist in animals and human infants as definite and specific responses to definite and specific stimuli. This is a seriously mistaken idea. Two misconceptions have led them to this erroneous conclusion. The first is the notion that the behavior of animals is definite and stereotyped. The other is the teleological conception of reactions in lower animals such as insects. Space will not permit lengthy discussion on this particular question. Suffice it to point out here that animal behavior is not so rigid and stereotyped as these authors have supposed it to be. In fact, variability and modifiability in animal behavior is the rule rather than the exception. (Of course, variability and modifiability are often limited by the action system.) As to the teleological interpretation of instincts in lower animals, we wish to merely mention the fact that the "miraculous" results accomplished by the insects are sheer accident, as has been shown in a great many cases by recent experimentation. (See Loeb, 'Forced Movements, Tropisms and Animal Conduct,' 1918, Chap. 18.)

[3] *Op. cit.*, pp. 652–653.

[4] *Op. cit.*, p. 101.

frankly admit that Professor Hunter's objection is a serious one. Indeed, unless we are able to meet this objection we may consider the anti-instinct view as an indefensible one. Feeling so, we think it is best to face the question directly and attempt an explanation for 'this somewhat sudden grouping of the elements into a significantly new response,' without any assumption of instinct.

There are at least four factors which determine whether or not a new response can be produced without a long process of learning. The first is the maturity of action system. If the muscles of a bird have not been well developed and if its wings have not been sufficiently feathered, it is obvious that no attempt would succeed in making it fly. Secondly, in order to produce an 'unlearned' act, the elements or the component activities of which the new response is composed must be sufficiently exercised and organized beforehand. The fact that a great many of our reaction systems can be acquired only after a period of exercise is in part due to inadequate preparation of the part-activities. Thirdly, the absence of antagonistic habits which will tend to interfere with the new grouping will account to a great extent for the ease with which the new response is produced. The whole process of learning is largely devoted to the exercise of part-activities and to the breaking up of antagonistic habits.[1] The number and the strength of antagonistic habits previously acquired determine to a great extent the rapidity of the grouping of the elements into a new response. Fourth, given sufficient preparation of the component acts and little interference of antagonistic habits, the new act will still not be at once produced if there are no sufficient environmental forces which tend to drive the organism to perform such a definite act. To test out whether a bird can fly effectively for the first time we must turn it loose in the air or suddenly remove its support, or have some other means to drive it away, or the bird itself at the moment must be seeing the

[1] We are here chiefly concerned with the problem of effectiveness of new acts so we neglect for the moment the problems concerning the accuracy and perfectness as well as automaticity of the act.

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flight of some other birds (imitation or enhancement of stimulus). To test out whether the moorhen can dive without previous training we must put it in the water. All such new responses, if they can be performed effectively without learning at all, must be regarded as the direct effect of the external situation and not as manifestations of certain innate tendencies. In other words, the new grouping will immediately take place and the new end result will be accomplished by the organism when the external conditions are such as to force the organism to act in that direction rather than in another.

It may be asked, in such cases, what pragmatic difference is there between the explanation we are offering and the conception of instinct? Answering this we can only repeat that the instinct conception is a 'finished' psychology. Suppose we try an experiment on the walking response of a babe under such conditions as to prevent the influence of habit formation. If it turn out that the babe can walk effectively under controlled conditions the instinct psychologist will conclude that walking is an instinct pure and simple. If it turn out otherwise, his conclusion will be negative (or in some cases, other than walking, he will say that the instinct in question has been modified or has waned). The result of the experiment may come out either way, but being primarily an experiment to determine the presence or absence of a given instinct, his investigation is bound to stop short there. On the other hand, for the non-instinct psychologists the mere statement that walking is innate or acquired is altogether unsatisfactory: if walking could be performed without previous training they would proceed to find out what has made this unlearned activity possible; they would, to repeat, investigate (1) the strength of the bones and muscles necessary for this walking response, (2) the amount of exercise and plasticity and perfectness of the organization of its component acts, (3) the presence or absence and strength of antagonistic habits, and, most important of all, (4) the nature and intensity of the stimuli and the total situation, which, acting together or serially, call out the walking re-

sponse. In other words the instinct psychologist ends his investigation where the non-instinct psychologist begins. Instinct, from James on, has had such a strong hold in psychology that it has prevented us from developing a technique for investigating the antecedents of our reactions. Our studies of habit formation have been so superficial that little attempt has been made to inquire into the genuine causes of the relative rapidity of new acquisitions. An act performed without previous exercise is an instinct; otherwise it is a habit and, in the latter case, our investigation always ends merely in the counting of the number of errors and the speed of the performance. We have seldom studied the basic reaction systems which underlie the organization of a new habit, neither have we analyzed in sufficient detail the stimulus factors which are so set as to force the organism to accomplish a definite end-result. One way to save this situation is, I think, first to dismiss completely from psychology this great wonder of instincts, and then gradually to establish a technique so that the degree of ease and rapidity of grouping of the elements into different new responses can be compared and their underlying causes discovered.

Restatement of the Hypothesis of Units of Reaction

The hypothesis of units of reaction which I suggested in my earlier paper may be restated as follows:

By units of reaction I mean the muscular movements of every part of the body of the new-born infant present at birth or shortly after birth. As illustration we may mention again such activities as sneezing, yawning, hiccoughing, various movements of the head, of facial muscles, eye muscles, of lips and of tongue, swallowing movements, crying and other vocal movements, the movements of the trunk and of the arms and legs in various ways, the extension and flexion of fingers and toes, grasping, sensitivity to sound, light, temperature, etc., and hundreds of others. Physiologically speaking, these units of reaction vary from one another in their complexity and in their neural coördination; that is, in the number of muscles and neural arcs involved.

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But from the standpoint of psychological behavior there is hardly any movement in the new-born babe which can be properly called an adjustment act to the environment, or, in other words, the units of reaction are not organized behavior-responses which serve biological ends; they are non-teleological or non-adaptive.[1]

In my previous paper I repeatedly emphasized the 'randomness' of the units of reaction. I meant by this simply to point out the psychologically unorganized and non-teleological character of the spontaneous activities of the new-born infant. But I unfortunately did not make it clear that my statement did not imply that any and every stimulus will call out every and all movements of which the organism is capable. In fact I am in full agreement with Professor Tolman that "their randomness is usually restricted within certain fairly definite and discoverable limits."[2] Certainly one stimulus will evoke one group of random movements which may not be the same as those called forth by another stimulus of a totally different nature. The movements caused by a loud noise are not the same as those caused by 'petting.' I am glad that Professor Tolman has emphasized this point, which I, though clear about it in my own mind, failed to do.

Throughout our discussion we seem to have been carrying an anti-heredity banner in our hands. The question will be naturally pressed upon us by our critics: Are these units of reaction, whether simple or complex, acquired or innate? In several places in my earlier article I have admitted that they are hereditary activities, but in a footnote I have called attention to the fact that habits begin to be formed even in the embryo, and that very possibly a great many of the units of reaction are the earliest and simplest habits of the organism.[3] But this is merely a conjecture. Our knowledge concerning the psychological life of the fœtus is too meager to enable us to ascertain the antecedents of the elementary

[1] In asserting this view I am neglecting for the moment those activities connected with vital functions. See 'Giving up Instincts in Psychology,' p. 658.

[2] *Op. cit.*

[3] *Op. cit.*, p. 658.

movements of the new-born infant, although we know definitely that the embryo begins its life as a behaving organism and receives stimulations, long before it leaves the mother's body, which have their effects upon the subsequent development of behavior. At present we would hesitate to make any dogmatic statement concerning the identity of those elementary movements which, from birth to a few days old, have the influence of previous exercise and stimulation as differing from those which have not. Theoretically we should like to see an embryonic psychology developed to settle this matter, but for practical purposes the question as to whether or not these units of reaction are innate is not of great consequence. We can begin our genetic study with the observation of these elementary acts, of course, carefully taking account of the effects of previous acts and their stimuli upon the units of reaction which appear later. (For example, the stimulation effect of cold air and pressure and the crying reaction upon the sucking movement.) [1] Our neglect of what has happened in the embryo will not do any injury to the scientific values of genetic psychology. Our hesitation in denying in unequivocal terms the hereditary character of the units of reaction may be taken by some as a sign of weakening in the claims of the thorough-going anti-instinct point of view. In spite of such a risk we should prefer to wait for future experimental support rather than to make a dogmatic assertion at present.

The random and chaotic movements of the new-born babe are the raw materials out of which our reaction systems are built. A most characteristic fact about them is their plasticity and their being capable of such varied combinations; under environmental demands they may be integrated and reintegrated into serviceable habits. From the thought activity of the great thinker of the age to our habitual acts of daily routine, all of our organized reactions have their origin in these elementary movements.[2] Habit formation

[1] I have borrowed this illustration from Professor Dunlap's article. *Op. cit.*, p. 93.

[2] Attention is called to the fact that not all of these elementary movements are performed at the beginning of life with equal perfectness; a great many incomplete

is simply an integration of these elementary acts into systematized responses or, as in a later age of life, reintegration of old habits. What is new in a new habit is the combination; its component activities can be traced back to the beginning of the life of the organism. Genetic study should start with the investigation of these random acts and follow the ways in which they are integrated and reintegrated into various reaction systems as results of environmental demands. The importance of these random acts has not been recognized by psychologists.[1] The conception of instincts which are supposed to burst forth from the organism from time to time without antecedents is largely responsible for this neglect. At present I believe the study of the nature of the units of reaction in the new-born child and the processes of their various integrations in the course of the continuous interaction between the environment and the organism is the key which will unlock the mystery of human nature.

The Acquisition of Instincts

If the anti-instinct contention and the hypothesis of units of reaction are to be accepted, it immediately demands of us an analysis and explanation of how our habits, usually called instincts, are acquired; that is, how they get integrated directly or indirectly from the units of reaction. We wish to call particular attention to the fact that all that is to be said below is merely a tentative suggestion and that we shall not claim any special validity until we have experimental evidences in our hands.

and feeble ones will have to be improved by muscular growth and exercise. But all the ultimate elements of our complex habits are present in the new-born babe, feeble, imperfect and rudimentary as some of them may be. It is also to be observed that the physiological functions of the reproductive organs appearing at puberty must not be taken as sudden appearance of sex instinct, or even as delayed units of reaction; they are purely physiological inasmuch as visceral functions and the function of other glands are physiological; they are important from the standpoint of psychological behavior only in so far as they serve as new stimuli (intraorganic) which often lead to formation of new habits, namely, sex habits. So far as the sex behavior proper and its preparatory reactions are concerned, all the elements have their origin dated back to the earliest history of the organism.

[1] Watson is the only exception, and, in fact, we are really adding nothing new to what he has emphasized. See *Behavior*, pp. 107–109, and *Psychology from the Standpoint of a Behaviorist*, pp. 270–272.

I

Simultaneous Integration

There are two types of integration, namely, simultaneous integration and integration in temporal order. By simultaneous integration I mean the grouping of the units of reaction directly or indirectly into a single organized response. Possibly this can be best illustrated by the acquisition of word habit. According to Mrs. Blanton's report, the following sounds are commonly heard during the first thirty days of the human infant's life—the consonant sounds are: "*m* in conjunction with *a*, as *ma* (at); *n* as *ngn* (nat); *g* as in *gah; h* as in *ha* (at); *w* as in *wah* (at); *r* as in *rah* (at); *r* as in *hurr*, very slight sound; and *y* as in *yah* (at). Vowel sounds are: *o* as in *owl; e* as in *fell; oo* as in *pool; a* as in *an;* and *a* as in *father* (relatively rare). She also observed a variety of cries as stimulated by pain, hunger, etc.[1] These cries and sounds are what we call random movements, not only in that they are meaningless, but also in that the neural-muscular mechanisms which produce these sounds are not well coördinated or organized. In acquiring a word habit, therefore, the neural-muscular systems such as those in connection with the larynx, pharynx, thorax, lungs, nose, palate, cheeks, tongue, lips and a great many other parts of the body must be integrated so that the sound of a given word can be uttered. During childhood these sound-producing neural-muscular mechanisms are very flexible and capable of numerous combinations so that sounds of different words can be easily acquired. In adults, however, the integrated word reaction systems become more or less fixed and rigid, and tend to interfere with new types of integration. This will account, in part at least, for the fact that it is more difficult for an adult than for a child to learn a new language.

All the organized single responses in our daily life are simultaneous integrations from the units of reaction. We mention our daily acts such as carrying food to the mouth, holding an object with the hands, picking up an object,

[1] 'The Behavior of the Human Infant,' etc., Psychol. Rev., 1917, 24, 457–460.

standing up, sitting down, leaping, kicking, stepping, balancing the body, embracing a person or an object, etc. Each of these acts, when performed, involves the coördination of the body as a whole although certain parts of it play the principal rôle while others act merely as subordinates (*e.g.*, in kicking, the legs play the principal part, while the other parts of the body involved are merely lending their coöperation so as to make the kicking movement effective). Unfortunately there has been no satisfactory laboratory study concerning the processes of the integration of the body parts into single organized responses. That there is imperative need for physiological analysis of the integration is apparent. Studies of this kind may be undertaken from two standpoints: we may study the simultaneous integration from the genetic or developmental viewpoint, or we may study it for the purpose of analyzing the physiological basis of learning certain particular types of activity.

Simultaneous integration may take different forms: (1) It may be merely an organization of the original units of reaction into unitary responses such as that which occurs when a child learns the hand-eye coördination, or learns to stand up. (2) It may be a combination of previously integrated acts into more complex ones. This type occurs more frequently in later years than in infancy. It is well illustrated by the process of learning to write in a child. The simple coördinations of eye-hand and finger movements are not sufficient to enable the child to write. Besides being able to hold the pen firmly in one hand, the other hand must press down the paper at the same time. Furthermore, both arms and shoulders and the head are also called into play while the trunk maintains the bodily posture and the eyes follow the movement of the hand; they must all work simultaneously. Now all these simpler elements or part activities are integrated acts in themselves. But when they are called to work together simultaneously in writing they form a new and more complex organized response. In my earlier paper attention has already been called to the fact that previously acquired habits usually furnish the basis for new acquisi-

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tions,[1] or, in other words, new habits are organized out of and upon old ones. (3) On the other hand, when the new acquisition is not compatible with the nature of the already acquired habits, not only the latter cannot be made use of, but may sometimes tend to interfere with the new organization. In that case we have to break up the antagonistic habits and gradually rebuild the new ones. Thus, when an adult is learning a new language, he always has great difficulty in pronouncing certain sounds which are not found in his native tongue. The simple explanation is that he has to break up his old vocal habits and reorganize new ones which can make little or no use of the former habits.

II

Temporal Integration

However, behavior does not occur in isolation; each act is always preceded and followed by some other acts. *Acts which are unfolded in serial order with a certain amount of regularity we call temporal integration.* Of great psychological importance is the serial or temporal integration in learning. Learning in most cases consists primarily in a rearrangement of single organized acts into a new sequential order. In other words, the units of reaction are integrated into organized single habits which are, in turn, integrated into various serial responses. Take the case of the rat learning a problem box or a maze. All the individual acts displayed by the rat in the maze from the moment he is put to the task until the habit is fixated are previously acquired, although exercise may tend to improve and facilitate certain individual acts.[2] When we say that the rat has learned the maze or solved the problem, we mean simply to imply that the animal has selected from the repertoire of its previously organized single responses certain individual acts and has hitched them together into a new serial order; as far as the individual acts

[1] *Op. cit.*, pp. 662–663.

[2] In emphasizing the importance of the temporal integration in learning we must not, however, lose sight of the fact that during the learning process simultaneous integration may be going on side by side with the temporal integration.

are concerned, possibly nothing new has been acquired even after it has reached the period of 'habit-fixation.'

We often say that a normal adult usually possesses thousands of different habits. This is true only when we speak of his temporal habits. But if we analyze his temporal habits into single or individual acts the number is not so numerous as we often suppose it to be, neither does one need to possess a large number of single acts in order to form his numerous habits. The striking fact is that there are hundreds of ways in which the individual acts can be arranged into serial order. The identical single acts may be organized in different sequences so as to produce different end-results. Language habits will illustrate this point very well. Think of the number of ways in which a single word can be combined with other words to form different sentences and think of how small a vocabulary one usually possesses, yet one is able to write volumes or talk for hours without repeating the same sentences.

There are several facts in connection with the temporal integration which must be discussed here:

1. Each series of acts may be divided into preparatory and consummatory reaction.[1] Such a division is solely for the convenience of scientific description;[2] it implies no teleology in the sense that the organism *is necessarily 'conscious' of the end-results* or that there is a certain *vital impulse* acting as a driving force of a purposive response. For the purpose of grouping various acts into series the psychologist will have to adopt certain objective criteria. One of the most convenient methods is to classify acts according to the end-results they accomplish. Hence we call those acts which appear to the outside observer to have inherent connection preparatory reactions with reference to the end-reaction. Arguing from the nature of the behaving organism itself, however, a plausible hypothesis can be postulated so as to lend support to this method of classification. This hypothesis

[1] *Cf.* R. S. Woodworth, 'Dynamic Psychology,' Columbia Univ. Press, 1918; also 'Psychology,' Henry Holt, 1921, Ch. IV.

[2] *Cf.* Tolman, *op. cit.*

has been suggested under different terms such as 'drive,' 'determining adjustment,' or 'driving adjustment,' or 'determining tendency.' This concept has been clearly discussed by several writers (Perry, Tolman, and Woodworth) and it is not necessary for me to enter into any lengthy description here.

I wish to point out, however, that on account of the terminology these writers have used, their position is very liable to be misunderstood and that certain of their statements are also likely to have unfortunate implications. The terms 'drive,' 'driving adjustment,' 'determining tendency,' 'directive tendency,' etc., as used by them, may be taken by some to mean some 'internal force' directing the behavior of the organism. Such a meaning will have some animistic implication and is apt to be associated with the vitalistic concept of McDougall and the Freudians, and will make this conception of purposive behavior objectionable as a mechanistic and objective explanation of behavior. Furthermore, in reading their writings[1] one often comes across such descriptive terms with reference to the 'drive' as *inner force, internal striving, directs activities, determines and controls lower propensities, releases energy, releases particular sets of acts*, etc. All such expressions seem to imply an active agency directing the action of the organism[2] and cannot be entirely free from the charge of being animistic, although, I think, these writers will resent this charge.

It is, therefore, necessary to employ a better term for this concept and interpret it in a more concrete and objective way. I should suggest that the term *behavior-set* can profitably be used for this purpose.[3] And by behavior-set I mean simply the *response-posture or anticipatory attitude which*

[1] *Cf.* Perry, R. B., 'Docility and Purpose,' PSYCHOL. REV., 1918, 1–20; 'A Behavioristic View of Purpose,' *J. of Phil.*, 1921, **18**, 85–105. Tolman, E. C., 'Instinct and Purpose,' PSYCHOL. REV., 1920, **27**, 217–233; 'Can Instincts be Given up in Psychology?' and Woodworth, R. S., 'Dynamic Psychology,' and 'Psychology.'

[2] The present writer has also been guilty of this somewhat unwise and objectionable description. See Kuo, Z. Y., 'The Nature of Unsuccessful Acts and their Order of Elimination in Animal Learning,' *J. of Comp. Psychol.*, 1922, p. 20ff.

[3] This term has already been occasionally used by Professor Tolman. See 'Can Instincts be Given up in Psychology?' *Op. cit.*

orients the organism in such a way as to make it react discriminitively and selectively to different stimuli or different groups of stimuli. At each moment the organism is acted upon by a great number of stimuli, but it cannot react to all of them at one time nor does it react to the same stimulus in the same way at different times. The particular act which the organism will produce at the moment depends upon a number of factors. Besides the influences of the setting and the nature and intensity of the stimulus, the historical relation between the organism and the stimuli, the nature and types of reaction systems the organism has possessed, and such factors as frequency, recency, and the like, the behavior-set plays a very important part in determining which act the organism will produce and which stimulus it will react to. It sets up a response posture or an bodily attitude in the organism towards a definite consummatory reaction (regardless, of course, of whether the organism is 'conscious' of the end or not). It forms a 'reaction tone,' so to say, which lowers the threshold value for those sensory stimuli which have more or less connection with the consummatory reaction and increases the threshold value for other distracting stimuli on the one hand, while on the other it tends to facilitate certain individual reactions and inhibit certain others. The behavior-set is not a driving force nor an impulse, nor an internal striving or craving, or an urge, and still less is it an active agency: neither does it control or release activities; it is merely a response-posture as such and nothing more. The reaction tone persists until the consummatory reaction is reached or until it is replaced or modified by some other reaction tone as in the case where the distracting stimuli are strong enough to force the organism to attend to them.

The above interpretation, while not changing the essential conception of purposive behavior set forth by the writers already referred to, seems to make the case more concrete and objective and hence less objectionable for the purpose of scientific description. While we have not yet had any experimental data to show its actual physiological changes,

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(and the whole matter will remain for some time a difficult research problem), the behavior-set can be indirectly inferred from external behavior. When we observe over and over again that each time a dog is not fed for a number of hours it becomes restless, acts as though it were seeking for something, makes no responses to certain stimuli to which it would ordinarily respond, and that these characteristic reactions will disappear after the food is given, we may infer that the behavior-set in the dog at the moment is what may be called food-seeking.

Thus far I have agreed in principle with the exponents of this conception of purposive behavior except that I have attempted to reduce this conception to more concrete and objective terms so as to preserve it from being tangled up with the animistic views of McDougall and the Freudians from which these authors are not free.

There is another point, however, at which I regret that I shall have to part company with these authors, especially Professor Tolman. Professor Tolman insists that the 'driving adjustment' is innate and not acquired. As I understand him, he is not unwilling to characterize his *driving adjustment* by such concepts as response-posture, or bodily attitude. Now the question is immediately resolved into this: Is a response posture innate or acquired? Before we answer this question we must make a clear distinction between organic changes such as hunger and thirst, and behavior-set or response-posture or anticipatory attitude (in this case the eating and drinking are anticipatory attitudes). The former are simply intraorganic stimuli which upset the equilibrium of the organism, while the latter are postural attitudes aroused by such stimuli when the consummatory reactions cannot be immediately reached.[1] Our question will be once more resolved into this: Can organic disturbance bring about a definite response-posture towards an end-reaction without any previous experience? Or, to put it more concretely, can hunger or thirst produce in

[1] In a conversation, Professor Tolman told the writer that he has been making the same distinction in his forthcoming articles, although he still maintains that the postural attitudes may be innate.

a child an eating or drinking response-posture if it has not eaten or drunk anything before? Can the disturbance of equilibrium produced by the secretion of the sex glands make the organism assume the posture of sexual act, normal or perverted, even if the organism has no sexual knowledge before? My answer is negative. Suppose a child is for the first time stimulated by hunger and feeding is delayed. The child becomes restless, displays various kinds of random movements until food is placed in its mouth and the disturbing organic condition is 'neutralized,' to borrow Professor Tolman's expression. In such a case it is inconceivable that even at the first instance of hunger the child will assume the eating posture before food is given; the hungry child is not necessarily the eating child. It is only by virtue of previous experience—that is to say, after a conditioned response has been established—that the hungry child will assume such an attitude or posture when the hunger stimulus is not immediately removed. The eating response-posture in the young infant consists of certain incipient sucking and swallowing movements which may sometimes become explicit and can be observed in some babes in hunger. Such definite implicit movements (or even explicit) without the presence of actual food stimulus, can come about only as a result of previous experience. That the eating response-posture is a result of the formation of a conditioned response can be further indirectly inferred from the fact that the mere contact of the nipple with the mouth will stop the cry of the hungry child of some experience, while in some older babes the voice of the mother in the distance will be sufficient to make the crying child quiescent. Other types of behavior-sets can also easily be shown to be results of conditioned responses. If we can devise a technique by which we may watch the behavior of an organism, man or animal, from birth on, we shall be able to tell definitely by indirect inference at any rate the genesis and development of behavior-sets, although a direct study of their underlying physiology will be extremely difficult. Let us hope that the time will soon come when this issue can be

settled experimentally instead of with merely verbal arguments.

To recapitulate: The *behavior-set* is a *response-posture* which forms the general *reaction tone* of the organism. It plays a very important part in determining the threshold value of responses but it does nothing else: it neither excites nor directs activities. This concept is to be distinguished, we wish to point out emphatically, from the *impulsion psychology* of McDougall and the Freudians, and all other mentalists who are more or less vitalistically and animistically inclined. Every and all behavior-sets are results of experiences and are not innate. The principle of conditioned response is applicable in the case of the acquisition of the behavior-set.

2. The arousal of the behavior-set, as well as each particular act of a series of the preparatory responses, depends upon specific stimulus patterns, intraorganic or extraorganic. The sight of prey or the contraction of the stomach wall may cause the lion to form a hunting response-posture. But, given this hunting response-posture, whether the lion will run or stalk or do something else to capture his prey depends upon the nature of the particular stimulus pattern at each moment. All precurrent reactions are very variable and their variation is partly determined by external stimulation plus physiological conditions. In the case of more or less fixed habit where the external situation seems to remain constant, the unfolding of each particular act of a series is conditioned by kinæsthetic cues, that is to say, the kinæsthesis of the preceding act serves as a stimulus which excites the subsequent response. The finger movements of an expert musician, dancing, writing, reciting of poetry or prose, are good examples of responses called out by kinæsthetic cues.

It follows that behavior-sets can be subjected to laboratory control: given an adequate stimulus, a definite behavior-set will be produced. This can be more easily done in animal behavior partly because of the relative simplicity of animal life. In our investigation of animal learning, the success of a given experiment depends to a great extent

upon our skill in controlling the behavior-set desired for the purpose of the experiment. In the human animal, however, we have not as yet developed any technique by which we may put his 'motives' so-called under adequate control.

3. In temporal integration the sequence of a series of acts may vary to a considerable extent as to the rigidity and definiteness of the order of their appearance. In few cases, however, are our habits so rigid and stereotyped that their segment acts always appear in fixed and definite order. The fixity and definiteness of the sequential order of acts depend primarily upon the frequency of exercise. The more frequent the exercise, the firmer will be the integration and the more rigid and fixed the sequence. We shall call those habits, the sequence of whose segmental acts are fixed and definite, *chain responses*. It is a very obvious fact that we often acquire rigid and firm integrations at the risk of becoming unadaptable to a new environment.

4. As corollary to 2 and 3 may be stated that the types of temporal integration (usually called instincts) the organism acquires are determined by the nature of environment, and that the strength of our acquired 'instincts' depends largely upon the *frequency* and *number* and *strength* of adequate stimuli which effect the organism. The first part of this proposition has been sufficiently discussed in my earlier paper. All I wish to do here is to point out that the so-called powerful instincts are really nothing but abstract names for those types of serial integration which seem to have common end-results and which are frequently found in the life activity of the organism. The frequent appearance of certain types of integration due to frequent stimulation often gives the instinct psychologists the impression that the organism possesses certain mysterious forces acting as directive agencies of behavior. For example, the great 'instinct of self-assertion' is merely an abstract name for those serial reactions which seem to serve *a common social purpose;* namely, the showing-off of the individual to his fellow men (although the precurrent acts combined to produce such an end result may vary from instance to instance).

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The reason why this instinct of self-assertion is so frequently observed is that in training the social behavior of the individual social approval and disapproval are the stimuli most commonly offered. If we count the number of instances in which society endeavors to bring about the 'showing-off' reaction, and consider the possible range of transference to other stimuli of which this reaction is capable, we need not seek for any mystical principle of inner forces in order to explain why the human animal has 'the will to power' or the dominating 'ego instinct.'

5. The process of integration is at the same time a process of selection and elimination of acts. In general those acts which contribute to bring about a consummatory reaction, and hence remove the exciting stimulus and reëstablish the bodily equilibrium of the organism, are more likely to be selected and integrated than others, while acts which tend to prevent or disrupt the consummatory reaction will be gradually eliminated. It must be borne in mind, however, that not all acts integrated in a series are serviceable to the end reactions; many excessive or superfluous acts may be selected and retained merely as a result of recency and frequency, or some other factors. As Watson has pointed out, "If the *extra-acts* were not fixed along with the successful ones, it is clear that we should be dwelling constantly in a world of virtuosi." [1]

Two more precautions should be taken before this view of selection and elimination can be accepted: (1) The so-called successful and unsuccessful acts are relative terms and are so classified only from the standpoint of the end-result which they accomplish or tend to destroy. Their successfulness or lack of success is not intrinsic in the acts; taken by themselves one act may be as successful and as important as another. Furthermore, the same act may be harmful in one series of integrations while serviceable in another. (2) Not all of our habits are acquired as a result of the long

[1] 'Behavior,' pp. 258–259. Italics mine. For a more complete discussion concerning this aspect of the learning process see 'The Nature of Unsuccessful Acts,' etc., *op. cit.*, p. 14 ff.

process of learning, nor is it necessary that all of them should go through selection and elimination. Many of our daily habits are established by merely casual and accidental contact with the environment, and may have no apparent biological service whatever. How troublesome it would be if we were obliged to acquire our manners and other simple daily habits with the same difficulty of adjustment and consumption of time which is necessary in the acquisition of a skilled operation!

To sum up: In the newly born organism, man and animal alike, there are found a number of psychologically unorganized and chaotic bodily movements—the units of reaction. These are later integrated into various reaction systems. Two types of integration can be distinguished, namely, simultaneous integration and temporal integration, the former being the organization of the body-parts into unitary responses; the latter, the arrangement of individual acts into sequential order. The elements or component acts in the temporal integration are previously established unitary responses. The types and nature of integration are primarily determined by environmental demands. We may classify temporal integrations according to the end-results they accomplish (and when we classify the types of temporal integration, we classify the *instincts*). The *behavior-set* concept is postulated to explain purposive behavior in purely mechanistic and objective terms. This concept is a basis upon which the proposed method of classifying temporal integrations is founded. The behavior-sets are developed through the establishment of conditioned responses.

INTELLIGENCE AND LEARNING[1]

BY JOSEPH PETERSON

George Peabody College for Teachers

In the last decade the progress and service of psychology have been closely related to the formulation and testing of conceptions of intelligence; and a noticeable shift has taken place in the evaluation of various psychological researches, depending in each case on the extent that intelligence measurement is involved. Indeed, men who have done very little investigation in psychology aside from the devising and standardizing of intelligence tests, have risen to prominence, and their advice and assistance not only in practical educational problems but in industrial and social work have been sought as was never before the service of psychologists. Learning problems which once occupied a field in psychology largely by themselves, only slightly related to individual differences, are now becoming so closely associated with intelligence measurement[2] that investigators are pointing back to the slight value of certain learning experiments once in good standing both in animal and human psychology; and we are becoming interested in questions of so-called general learning capacity and in the common factors of various learning processes roughly in proportion to their promise of throwing light on the nature of intelligence. The accumulating evidence for the now prevailing view that one's intelligence remains practically constant through life, is bound to contribute to the enhancement of interest in general mental ability.

[1] Presidential Address, Southern Society for Philosophy and Psychology, Memphis, Tenn., April 14, 1922.

[2] Peterson, Jos., 'Experiments in Rational Learning,' PSYCHOL. REV., 1918, 25, 443–467; 'The Rational Learning Test Applied to Eighty-One College Students,' *J. Ed. Psychol.*, 1920, 137–150. Sunne, D., 'Tests of Discrimination and Multiple Choice for Vocational Diagnosis,' *Psychol. Bull.*, 1919, 16, 262–267. Hunter, W. S. 'Correlations of Maze and Otis Examination,' Abstracts of Papers before the American Psychol. Association, 1921, 24f. Haught, 'Interrelations of Some Higher Learning Processes,' to appear as *Psychol. Monog.*

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We are still less than a century away from the death of Itard, who, after the pathetic failure to educate the 'savage of Aveyron,' exclaimed: "Unfortunate! Since my pains are lost and my efforts fruitless, take yourself back to your forests and primitive tastes; or if your new wants make you dependent on society, suffer the penalty of being useless, and go to Bicêtre, there to die in wretchedness."[1] This exclamation was made when he first suspected that his savage might also be an idiot, a possibility that Pinel had warned him against but which he had refused to believe. Though he believed in the incurability of idiocy he failed to see the intermediate grades of intelligence between this condition and normality, and the corresponding limitations of development.

It is, of course, not to be denied that individual differences with regard to general intelligence drew attention in earlier periods of psychology; so patent a fact of human nature could not escape the notice of keen observers. But there were also strong influences tending to distract attention from these differences. Politically the atmosphere was full of the doctrine of the equality of man, and this doctrine, though ostensibly aiming at the equality of opportunity, was too freely applied to innate capacities as well. It is impossible in practice clearly to separate the two, for innate differences cannot command equal opportunity. Conceptions of justice as reward according to one's works; of salvation in some other world on the basis of such justice, making due allowance for late repentance; and the practice of indiscriminate punishment according to the deed, with little or no consideration of the condition of the doer of the deed and no adequate evaluation of the circumstances of its occurrence,—all these conditions operated powerfully against the active study of intelligence differences. Then, too, scientific methods for such studies had not been developed. There was also the futile and deceiving question of the freedom of an arbitrary will, which had not yet lost

[1] Quoted from Seguin, 'Idiocy and Its Treatment by the Physiological Method,' p. 22.

its vitality, as every mis-stated and ill-conceived problem some day must do. Philosophy occupied the field and claimed as its own, problems of conduct which are now yielding to more mature scientific methods.

When psychology relaxed its preoccupation with faculties and later with the psycho-physics of sensation, it took up the dissection of consciousness into elements and the classification of persons according to imagery types, etc.; it studied simple reactions in great detail and classified reactors into sensorial and muscular types; and it listed numerous introspective differences often based on so few cases, indeed, and dependent on such immediate control as to be devoid of scientific reliability. The simpler processes were more amenable than complex ones to scientific control, and so received the major portion of attention in the early experimental investigations. It is undeniable, of course, that many important psychological facts and methods pertaining to more complex phenomena have their origin in these early experiments.

It is probably *à propos* to mention here also the fact that the old adage *mens sana in corpore sano* has largely lost its force with psychologists in our day, except for extreme abnormalities. Some of the most sickly men have been great thinkers and students, and have contributed much to intellectual progress, while a very limited mind *in corpore sano*, as the term is usually taken, is a common spectacle, even in our universities. The modern school psychologist does not begin his diagnosis of misfits and retardates in the schools by detailed examination of physical defects and of vision and hearing; for he knows that frail persons and even blind or deaf-mute children may rise to prominence intellectually. However, good eyes and ears still have their great value, and robust health is rightly coveted, even if they do not directly condition mentality. The evils of adenoids and tonsils, of malnutrition in its various forms, of poor nerves, defective heart, etc., are patent, and they are here acknowledged fully as important conditions to study and eliminate as far as possible, in order that there can be no room for

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misunderstanding. The psychologist can but regret that the diagnosis and proper treatment of these physical defects by physicians is yet so qualitative and uncertain in many respects, so unsatisfactory for scientific study in relation to effects on intelligence, and financially so far out of the reach of many children.

While noting that these shifts in values have occurred, we must not neglect also to point out that the meaning of the term 'intelligence' has in late years been extended somewhat to include more of what was at one time termed the active powers of man as well as of the feeling life. Intelligence was formerly closely associated with intellect, as used, for instance, by Bain, following an old practice. Thus we read in *The Senses and Intellect*, published by this author in 1856: "The primary, or fundamental attributes of Thought, or Intelligence, have already been stated to be, Consciousness of *Difference*, Consciousness of *Agreement*, and *Retentiveness*. The exposition of the intellect will consist in tracing out the workings of these several attributes."[1] This general compartment of the mind had been treated by Locke and his followers as the Human Understanding (usually in capitals), and it comprised particularly the cognitive processes, involving, at their highest reaches, judgment and reasoning. This conception of the mind was closely related to the Wolffian view of faculties or powers of the soul, except that there were fewer such faculties than Wolff described. It is true that several writers attempted to account for the operations of the Understanding or Intellect on the basis of the 'association of ideas,' but even as careful an enquirer as Locke allowed an active reflective power to creep in, probably somewhat surreptitiously.

The early intelligence tests were a natural consequence of this background, which placed the intellect in the cognitive division of the mind, and were directed at certain of the several intellectual functions; such as discrimination, attention, sensory acuity, memory, and others. Binet's great success in mental testing lay in the fact that he departed

[1] Quoted from Rand, 'The Classical Psychologists,' p. 483.

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largely from the view of testing separate functions, and his work was the germ of the present-day interest in, and preoccupation with, intelligence measurement. He was well aware, however, that the several mental functions are involved in different proportions in his various tests. There are signs, indeed, that psychological testing, when much more perfected than at present, may again revert to the practice of testing separate functions. Binet observed three characteristics of successful thinking at the solving of problems, which characteristics he supposed to have important relations to the general degree of intelligence. These are (1) the tendency to take and maintain a certain direction, to select out and react continuously toward the essential aspects of the situation with respect to this direction of movement; (2) The ability to make the adaptations needed by varying the choices, the means employed, and the ideas for the attainment of the desired end; and (3) the power of autocriticism, or the ability to put a sort of objective evaluation on one's own efforts.[1]

The practical work of testing and the many useful applications of results thus obtained have brought us face to face with numerous problems the solution of which will undoubtedly occupy psychology for some time. For instance, since it is obviously impossible to test pure native endowment, whatever its nature may be, so that testers are limited to the measurement of degrees of performances based to a greater or less extent on acquired knowledge and habits, it has been necessary to assume that under an approximate equality of experience the brighter the child the greater the acquirement. That is, we have been forced to assume that there is almost a perfect correlation between degrees of ability to learn the habits and ideas on which the tests are based and degrees of intelligence; and yet this is just one of the problems that we must investigate. There may be various other characteristics, besides general learning ability, that make for successful behavior of the kind that we recognize as manifesting a high degree of intelligence; for example,

[1] Binet and Simon, 'L'Intelligence des Imbéciles,' *L'Année Psychologique*, 1909, 128ff.

general energy, pleasantness of manners, patience, persistence, self-control in danger and in irritating circumstances, co-operative tendencies, etc.

Woodrow has shown in an experiment on groups of inferior and of normal children, that in certain forms of learning children of different ages but of equal mental age, as this term has been defined by Binet, learn at equal rates; but we are at a loss to understand the conclusion that Woodrow himself draws,[1] and that others[2] have drawn from such experiments. They have concluded that equal mental age means in general equal ability to learn, irrespective of actual age or of intelligence quotient; that an older dull child has at a certain stage of his development equal learning ability to that of a younger bright child of equal mental age. But inasmuch as the mental tests determining the mental ages were themselves chiefly measurements of certain acquirements gained by the younger child in fewer years than by the older (defective) child, some of us fail to see the logic of this conclusion. Why, to make this problem concrete, should one child in ten years of life have acquired the facts and habits (along with the intelligence) that give him a mental age score of twelve years, say, while the more unfortunate youth of fifteen years, who also has a mental age of twelve years, has been fifty per cent more years in acquiring the same group of facts and habits,—*if* both have the same learning ability at equal mental ages? Have they not both come up through all the lesser mental ages, and has the duller one not taken three hours at each stage to the brighter one's two hours? While all the Binet tests are not based equally or entirely on learning ability, so that there may be other factors to consider, it does not seem that we are yet justified from experimental results in making any such wholesale generalizations as to the relations of intelligence to learning ability in general. The learning processes in Woodrow's experiment were mostly of a somewhat mechanical

[1] Woodrow, H., 'Practice and Transference in Normal and Feeble-minded Children,' *J. of Educ. Psychol.*, 1917, 8, 85–96; 151–165.

[2] See, for example, Hollingworth, 'Psychology of Subnormal Children,' 1920, 178ff.

nature, such as segregation of wads. In the Jesup Psychological Laboratory we have a few results on the Rational Learning test, a sort of multiple choice learning involving higher mental functions, which contradict Woodrow's results, showing much greater ability to master the problem on the part of the younger, bright child.

It, of course, may turn out on further research into the matter that a very young child learns as rapidly to segregate wads as does an older child or even an adult. Woodrow's whole experiment is based on the assumption that the angle which the rising curve of learning makes with the base line increases directly with increase in mental age. If this assumption should be found to be invalid—if young normal children should be found to learn in the performances used in his experiment as rapidly as older normal children, within certain age limits—his experiment would have little meaning. We must evidently find out more about the matter experimentally.

Another problem that test-results have raised is that of the general nature of mental growth. It has definitely been established that growth in mental ability ceases at a comparatively early period in life, just as growth in height does. The age at which this mental maturity is reached is not established, but it undoubtedly is not far from sixteen years. That is to say, at the age of about sixteen years each of us has all the intelligence he will ever have; only experience can be added. A few investigators now put mental maturity considerably below this age. It has been argued, for instance, that since the average mental age of the army recruits has been found to be about 13.4 years,[1] mental maturity must be reached at about this age. However this argument is probably based on an error. A large proportion of the army recruits had developed habits very adverse to efficiency in the sort of tests given, tests in which ability must be shown by reactions with a pencil to words, to problems, to pictures, etc. Many of the men were very unpracticed in the use of a pencil and in rapid calculation, as well as in the reading ability that was necessary to give them quickly a clear idea

[1] *Memoirs Nat. Acad. Sciences*, 1921, 15, p. 638.

of the nature of the test problems to be solved. It is known that training in speed in such performances may in a short time show marked effects toward increasing efficiency, but we do not know just how much this effect is in the different performances involved in the alpha and in the beta tests. It is certain that these men could have scored higher if they had been taken at a period of mental maturity preceded by a fair amount of practice. Their habits, on the contrary, worked negatively under the conditions of the tests. Evidence of this position is seen in the fact that those men with clerical habits scored comparatively high—higher, when compared with the different vocational groups, than would be expected on the basis of any selection factors known.[1] Then, too, we must remember that the recruits were probably not a fair sampling of the adult American.[2]

At present it seems probable that intelligence growth is rapid in infancy and that it gradually approaches its limit, probably asymptotically; but this has been denied by some investigators. The difficulty in solving this problem lies in the fact that scores are differently interpreted. It is obviously an error to treat absolute differences in raw scores as if they directly and exactly reflect differences in intelligence; the scores can only approximately represent the intelligence, and the approximation at different stages will vary for different sorts of data, and especially for different methods of administering and scoring the tests, for time allowances, and so on. For instance, *the decrease in the amount of time* required to do a piece of work with increasing age, will give a measurement if plotted in absolute units, as some writers have done, different from that given by the *increase in the amount of work done* in a fixed amount of time, or even in as much time as the subject desires to take. It is really a difficult statistical problem to get a fair measure of the growth of intelligence through different periods of time.

[1] This criticism of the interpretation placed by some psychologists on the results of the army intelligence tests applies to both beta and alpha tests in the main, but it does not, of course, question the great value of the tests for the purposes intended.

[2] Terman, L. M., 'Mental Growth and the I.Q.,' *J. of Educ. Psychol.*, 1921, **12**, 325–341. See particularly p. 330.

Certainly the plotting of the increase in raw scores with successive years of age[1] is a poor substitute for a real solution; it assumes equality of units for all ages.[2]

The growth of intelligence of bright children seems to differ from that of dull ones in being proportionally faster throughout the years of growth, which years are probably about equal for all children. The view of some psychologists that growth rate in the two groups is about equal for a number of years, but that the dull or the feeble-minded persons come rather suddenly to a limit and stop growing earlier than the normal and the bright children, does not seem to be supported by the results of extensive intelligence testing. It is true that pathological cases may be selected to illustrate the view of 'arrested development,' but this view is certainly losing ground as a result of tests of fair samplings of children at various ages.

Altogether, it seems to me, we are today falling into the grave error of identifying the intelligence quotient, as measured by certain mental tests, with intelligence. The statement of results in terms of this quotient is only a convenient way of presenting in a simple ratio the summary of a child's scores on all the tests given him. This device, useful as it is in practice in simplifying our statements of results, has connected with it the dangers of any partial presentation of facts: it focuses attention on some one aspect of the results, an aspect that can be stated in a single number, and it leads to the neglect of numerous important differences among individuals, and, what is worse, it closes the minds of investigators and of many practical users of the tests to such extremely important characteristics as subjective mindedness, degrees of emotional instability, methods of attack on problems, continuity of purpose, and to many other traits that tests have not yet been able properly to evaluate. Indeed, every psychologist with even a little clinical experience has probably found persons of normal or even high

[1] Freeman, F. N., 'The Interpretation and Application of the Intelligence Quotient,' *J. of Educ. Psychol.*, 1921, **12**, 3–13.

[2] Kuhlman, F., 'Results of Repeated Mental Reëxamination of 639 Feebleminded over a Period of Ten Years,' *J. of Applied Psychol.*, 1921, **5**, 195–224.

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intelligence quotients who are far less capable mentally than others of comparatively low quotients. It is fortunate that we have simple measures of persons' mental ability, measures that work so well with the common run of humanity in educational and industrial enterprises; but these conditions must not blind us to the great variability from individual to individual in mental make-up, which any detailed study reveals, nor to the caution necessary in diagnosing special cases of bright as well as dull children. The results of modern research indicate that intelligence is by no means a simple factor; and these results seem to promise a return to the special functions type of test for purposes requiring a more thoroughgoing analysis of individuals. It would, indeed, be unfortunate if the scientific work of testing and of analyzing human character that is so complex as to reward any degree of detailed study, should degenerate into a mere mechanical routine, because of its practical usefulness in simplicity.

In the mean time the army intelligence tests have clearly revealed the fact that the native intelligence of a nation is one of its greatest assets, one that is well worth scientific investigation and development as well as adequate training. The work of bringing about a recognition of this fact rests chiefly on the psychologists of the country, and it is doubtless one of our most important duties in this generation.

It is undoubtedly true that degree of intelligence is somewhat closely related both to the rate and to the limit of learning, especially when higher forms of learning are concerned. Learning is adaptation to external conditions, and intelligence is certainly to be judged in considerable measure by the nature of such adaptation. But a wholesale identification of learning ability with intelligence is confusing, to say the least. In the first place, ability to learn in one line of performance or on one kind of problem does usually not mean correspondingly high ability in others. While as a rule even the raw coefficients that have been obtained from inter-correlations between average grades in various pairs of school subjects, as determined, for example, by

Starch, show fairly high correlations, nearly as high, indeed, as the correlations between the grades in the same subjects in successive years could be expected to be, it must be remembered that numerous similar situations and common elements doubtless exist between different pairs of school subjects. The fact that in school work history is one division in the curriculum and geography another, should not blind us to the obvious truth that in the main these studies are much alike, each being complex compilations of data involving on the part of the student numerous responses of varied nature. In this respect, even arithmetic is not so peculiar a subject as is usually supposed. All lines of school work are complexes of various kinds of situations, involving in part responses to symbols, in part responses to concrete objects and situations, and to a considerable extent also responses to the behavior of other persons, the instructors; and in each of these cases some responses must be immediate while others are delayed. Indeed there is probably as much correlation between the situations presented by different instructors irrespective of subjects, as the reverse; and the different subjects of the curriculum are really not separate kinds of learning processes.

In a recent book Pyle, somewhat confusingly, it seems to me, and with varying consistency, attempts to identify the different learning processes in a general learning capacity. Referring to the inter-correlations of several such processes, ranging from marble sorting to the well known substitution test in different forms, which inter-correlations in most cases lie near .50, he says: "In the author's opinion, if the disturbing factors could be eliminated, the correlation would be unity." "There is a human characteristic," we are told, "which we may call general learning capacity, just as there is another which we may call general intelligence." But this general learning capacity 'never operates entirely freely but is always complicated by specific factors, and by the effect of previous experience.' The specific factors seem to be such as pitch discrimination, judgment of harmony and of consonance, visual acuity, color vision, degree of muscular con-

trol, perception of rhythm, etc. Such a view, which, as we just mentioned, the author does not seem to maintain consistently throughout, is certainly far from satisfactory for our purpose of finding relations between intelligence and learning. In certain chapters of this book the reader is made to feel the close relation of the 'general learning capacity' to general intelligence, and yet we are told, as the quotation above shows, that the two capacities are different. Why should each thus be conceived as an individual, if not discrete, thing? The quick learner, we are further informed, "has what is called in ordinary terminology, *the power of concentration.* All the available energy seems to participate in the learning; there is no waste of energy. This characteristic is what is usually spoken of as *power of attention.*" The 'ability to perceive significance' is given as another characteristic affecting learning capacity.[1] Thus we find ourselves confronted with a view of learning capacity as possessing certain powers though unitary in itself, a view not much different from the older faculty view, and one, unfortunately, that permits of a jump from the *fact* of .50 correlation to the *belief* in perfect correlation.

As a matter of fact, in a recent study by Haught on the inter-relations of higher, presumably rational, learning processes,[2] no very high correlations were obtained, and a partial correlation analysis revealed the absence in certain of the processes of factors present in others. If the higher learning processes involving rational behavior, when stripped of the numerous common situations presented by different school subjects, do not yield high correlations or show close inter-relationships, it is certain that learning ability in general is a myth. Indeed, very low correlations are not infrequently met.

We evidently need a more functional analysis both of learning and of intelligence; for recent investigations have revealed also the lack of a general intelligence factor common

[1] Pyle, W. H., 'Psychology of Learning,' 1921. Quotations are from pages 161, 168, 169, 170.

[2] To appear as a Psychological Monograph.

to the various intellectual operations involved in different mental tests, such, for instance, as Spearman has postulated. Indeed, when we apply mathematically the 'best scoring method' to batteries of group intelligence tests, we find that to get the highest correlations with the several criteria for efficiency in different lines of endeavor, we must score rights and wrongs differently for different purposes and vary the weightings of the several tests in the battery. The combination of traits that makes up what we call the highest intelligence in one vocation or line of endeavor is not the combination that gives the highest intelligence in another. There is, however, no occasion to deny that some individuals measure up well from nearly all standpoints while others have low mentality in all lines. The intelligence of any given individual seems therefore to be a complex of several abilities, none of which may be entirely absent in any individual; but the relative emphasis of any one factor varies from individual to individual. General intelligence, in the strict sense of the term, is probably only a fiction of our thought.

What then is intelligence if it is not simply learning ability? Probably the relations of intelligence and learning are really more intimate than Pyle's statement, that each is a separate general capacity, would lead us to suppose. Too frequently learning has been regarded as a sort of passive adaptation to environmental conditions, or an adaptation to *immediate* circumstances. This view is at any rate often implied, if not explicitly asserted, and the learning in such cases is supposed to be determined by the frequency and the recency of the stimuli that the individual has encountered. Learning on such a view is wholly contingent upon the order (and perhaps the intensity) of the stimuli that happen to impinge on the individual. This is the passive deterministic view that seems to be forced upon many psychologists today, even though there is beginning to be more emphasis placed on the instincts. However, this more dynamic view of instincts as movers to behavior is meeting some difficulty in the attempted amalgamation of it with the passive association view. While the instincts may serve as drives, the

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frequency-recency doctrine will still fix their *drive-ways* along channels that are determined wholly by the mere contingencies of the immediate environment.

Wherein lies the weakness of this passive association view, which certainly belies our common observations and introspections? It seems to lie chiefly in the rather common assumption that because the nerve impulse has great speed and flashes through the body in a small fraction of a second, the excitation of any one stimulus therefore dies out quickly and the individual comes under the spell of the next succeeding stimulus. Of course the usual view is not so boldly stated as this. The fact probably is that we are never under the influence of a single stimulus, but are always directly reacting to several stimuli, some simultaneously impinging upon our sensory end organs and others occurring successively. Somehow the effect of successive stimuli overlap so that we may react to several things at once; that is to say, we react to situations and not to mere successive individual stimuli. The fact here stated is undeniable, and is illustrated in a very simple form in temporal rhythm in which the separate impulses come so rapidly in succession that our muscular movements cannot easily respond to each separately, so we react with foot, head, or hand, to groups of two, three, or more impulses, as the case may be. More complex cases also occur, as is illustrated in responses to some problem wherein the meaning or the end is perceived; but such cases have not been sufficiently studied scientifically. How such behavior is possible neurally is one of the big present-day problems of our science and of functional neurology; but that it is possible is evident in almost any moment of our behavior, either introspectively or from the standpoint of external observation. The reason that the problem has come upon us so forcibly only today is that we have been using such terms as perception of meaning, perception of end, perception of relations, will, attention, judgment, and others, as explanatory. This subjective conception of different faculties which carry with them the implications of arbitrary forces, has blinded us to the real problem. The passive

associationism satisfied us only because its inadequacies were covered up by these faculties. Examine most of our modern texts and reports of investigations, and see if we are free in our thinking from the use of arbitrary faculties! A departure from the use of these arbitrary faculties by some writers has left them with only the passive frequency-recency conception of learning.

The inadequacy of this passive view of learning, based on an over-simplified neurology of behavior, is so patent as to be observed even by the layman; so while the more scientific psychologist has either admitted frankly our present ignorance regarding the more complex aspects of behavior or has supplied the deficiency of neurological knowledge by the free use of such terms as perception of results, consciousness of relations, voluntary direction of acts, etc., the less scientific person, the tender-minded, to use a term employed by Dunlap, has freely drawn on animistic principles, and on 'the subconscious,' usually capitalized and used in the sense of a conscious unity with arbitrary powers, outside the reach of our conscious life. Anything that ca n not be explained as due to the normal processes of our waking life is referred to the subconscious.

All such 'explanations' have, of course, only dodged the issue; but how can we as psychologists blame the non-scientific person for this when we ourselves have been ascribing the same kind of powers to consciousness, whenever our neural account has come short? I have in my possession a quarterly publication by one of our own Southern state universities, a commencement address by an honored citizen (but not a psychologist) which extols, apparently with the approval of this institution, the wonders of the 'subjective mind.' A few sentences will give the point of view: "It is in the silence that we conceive and visualize that which later takes form and is manifested in the external world. This is true of every great work which man has accomplished along any line. The processes go on in the subjective mind which never sleeps but is always busily engaged. A problem

arises in the objective mind and demands a solution. It is relegated to the subjective mind which is capable of solving any problem which is properly presented to it. We are not conscious of it, but the work is constantly going on until finally the solution presents itself to the conscious mind." What an opportunity we are missing, my fellow psychologists, by not letting this hidden, sleepless genius solve our problems, too! Note how our pseudo-scientific brethren, the Freudians, by invoking the aid of this wonderful *subconscious mind* and its more sleepy and less capable antagonist, the 'censor,' have succeeded even beyond their original expectations, with our own psychological problems as well as with medicine, philosophy, mythology, art, morality, and other fields! The writers representing this dogma, accepting it uncritically on the all-or-none principle, have succeeded in finding in nearly all lines of human endeavor, illustrations suitable and convincing to themselves of the workings of the subconscious. Of course a collection of such substitutions of terms for causal relations and of such uncritically selected illustrations interpreted to suit the theory, is not science at all.

While psychology cannot hope to satisfy the popular mind which can give no serious attention to its foundations and which expects simple answers to its problems, in its own familiar terms, there can be little doubt that real deficiencies in the science are in part responsible for the pseudo-science illustrated above. James felt the deficiency keenly and wrestled continuously with the edicts of a too-passive determinism which the associationism of his time forced upon him. He was driven, because of his keen introspective powers, to hold (though not consistently) to a 'scientific psychology' in the text of his 'Principles,' and to a contradictory sort of ethics in his footnotes. Surely when we shall know more of the facts about the causal connection of stimuli of all kinds and their resulting responses in various complex forms we shall not need to hold that ethics demands any other basis in human behavior than that of facts. The right must somehow be the consistent, the greatest moral economy,

to use an apt term of Perry's, or else *why* is it the right and *whence* come its sanctions?

It is desirable that we do not get our hypotheses of the underlying neural processes in learning so over-simplified for purposes of explanation as to arrive at a synthesis untrue of the complex delayed responses so characteristic of our ideational behavior. The mind is by no means like a phonograph, as one psychiatrist has asserted it to be, bringing up experiences in the order of their original occurrence. In our higher rational acts we receive stimuli, both simultaneous and successive, from the various aspects of a situation, and then we work over the impulses somehow into responses that are more or less consistent with all the circumstances affecting us. That this is the case has been recognized for centuries, but when we come to the point of making explanations in terms of the nerve impulses, we find ourselves up against some really serious difficulties. How are such delayed responses possible since it is known that the nerve impulse flashes through the whole length of our bodies in a very small fraction of a second?

Too frequently behavior is regarded as merely of the reflex type: a stimulus is received from external objects and a response results; another stimulus, and another response; and so on. How do the impulses become integrated into a unitary plan of action? While it may be true that all integrations may some day be understood on the basis of merely the greater frequency of the propagation of nerve impulses along certain tracts than along others, such an interpretation is far from promising today. It is discounted by many familiar facts of behavior in the form of facilitation and inhibition of one group of impulses by other groups, usually explained, if at all, on the assumption of some sort of drainage. The integration of neural impulses making for unitary action of some sort in the central nervous system, as reflected in certain phenomena of attention and voluntary choice, will furnish problems for neuro-physiologists for some time.

There is perhaps no more pressing problem in psychology

just now, nor a problem more pregnant with useful suggestions on the practical work of education, than that of how errors become eliminated in trial and error learning while certain acts finally survive in habits, to the exclusion of others. All sorts of hypotheses have appeared, but none has been generally accepted as demonstrated. Lloyd Morgan's early suggestion of fixation of 'successful' acts by pleasure has its modern advocates. Thorndike's 'law of effect,' closely related to this view, is but a statement of facts that every one knows in a general way, not an explanation at all. The 'law of combination' stated by Woodworth,[1] who recognizes the inadequacy of the usual association formula, is a more definite attempt to get at an explanation, though it is not worked out much in detail.

The frequency doctrine derived from passive associationism has wide currency in the literature of modern psychology, but as applied to learning this doctrine is usually so vague as to evade technical experimental verification. To some writers repetition means only repetition of trials; to others, repetition of stimulus conditions; and still to others, repetition of certain acts, a thing that is impossible before the acts have been learned. Obviously the repetition of nerve impulses through certain tracts under constant conditions of control is a vastly different thing from repetition of some unlearned *act* in the early stages of the learning; and even if externally controlled conditions brought about a repetition of the act, there would be no assurance that the nerve impulses bringing it about would be exactly repeated. The case of recency, as well as that of vividness, is not much better, so far as clearness goes in psychological literature, each of these terms being likewise fraught with ambiguities.

Most experiments on the effect of frequency and recency have failed to fix upon any constant meaning of these terms, and are so much controlled by other considerations that the learning can in no sense be attributed to these factors. In one investigation,[2] for instance, in which frequency and

[1] Woodworth, R. S., 'Psychology,' 1921, 398ff.

[2] Hamilton, G. V., 'A Study of Perseverative Reactions in Primates and Rodents,' *Behav. Monog.*, 1916, Ser. No. 13, p. 40.

recency factors are considered, frequency gets credit for casual influence in learning, whereas a careful consideration of the data clearly shows that the responses in accordance with frequency expectations come in increasing ratio as learning advances, as a *result* of the habit being formed. More recent studies,[1] better controlled so far as these factors are concerned, indicate that frequency-recency influences in themselves are as apt to fix a wrong as a right act, and that, in certain forms of learning at least, they actually have no selection value at all. Indeed, it has been shown that constructive learning may go on directly against expectations based on frequency and recency influences. In one case the situation was so arranged that frequency and recency factors in the stimuli were exactly balanced for and against learning, but in the responses they were both 1024 to 1 *against* learning, according to the law of probability, and yet learning was not hindered.[2] By actual count of the responses made by the subjects, it was found that conformity with the expectations of frequency and recency was about as usual in the wrong as in the right responses. In such cases one may object that reasoning or intelligence enters to overrule these impending factors. Such an objection, if the influence of consciousness as an arbitrary force is implied, would reveal just the condition that we have been deploring, that as soon as our passive associationism fails we are ready to invoke mystical forces to supplement it. Under such conditions we can expect little advance in scientific psychology.

It appears that our conception of learning needs modification, particularly as to the alleged dependence of the process on frequency-recency factors. Elimination of random acts not favoring the dominant determining tendency seems to be brought about somehow by interference through conflict of different interacting processes. Determining tendencies and purposes are themselves but general directions

[1] Peterson, Jos., 'Frequency and Recency Factors in Maze Learning by White Rats,' *J. of Animal Behav.*, 1917, **7**, 338–364; also Kuo Zing Yang, 'The Nature of Unsuccessful Acts and Their Order of Elimination in Animal Learning,' *J. of Comp. Psychol.*, 1922, **2**, 1–27.

[2] Peterson, Jos., 'Learning when Frequency and Recency Factors are Negative,' in press. A preliminary report is abstracted in *Psychol. Bull.*, 1921, **18**, 80 f.

in behavior effected by the larger consistency of the environment and by the inner metabolic processes. The more consistent acts survive in the form of ideals, principles, vocational activities, etc., and the less consistent ones are eliminated, not because the former are stimulated more frequently and the latter less frequently, but because of a greater degree of mutual facilitation by the several part processes in the one case and of inhibition in the other. Many stimuli, both external and internal, affect the organism simultaneously, and since, particularly in the higher organisms, there are various interrelated organs so arranged that each stimulates others, it is obvious that any important effects may be reflected from organ to organ, each being an external stimulus in fact to the other.[1] Often the stimulus of one organ or part process by another is not direct (mechanical), but one organ may set up an internal metabolic change the effect of which becomes a stimulus to another. Skeletal muscles, in responding to any stimulus, also excite sensory impulses (proprioceptive) which tend to arouse further responses. Thus effects of stimulation by any external object are held over in the organism for a time to blend with other later stimuli, and the result is that the organism reacts to situations involving many objects, and the part processes by mutual inhibition and facilitation gradually take directions giving them on the whole the completest expression. This account agrees very well with introspective evidence. At any one time in a complex situation we not only see many things to which to adjust ourselves, but we clearly recall various circumstances in the immediate past with which our reactions must also square.

In complex, conscious organisms like ourselves these part processes if strongly organized in certain directions may be felt as original forces compelling us to act in certain ways, or to inhibit certain desires or other tendencies. What we call will is evidently the result of such organizations, as are also certain automatisms which may seem to prevail over

[1] For an interesting and suggestive contribution from this point of view see Goodrich, Edwin S., *Science*, N. S., 1921, **54**, 532 f.

the will at certain times, with the feeling that other personalities are controlling our own acts. Double personalities are thus to be understood. Learning thus considered is getting control over immediate conditions for larger and more far-reaching orientation, and is not merely adaptations to the contingencies of immediate stimuli.

Such a view presents the old question of freedom in a light that is suggestive and of practical value, as well as one that is consistent with science. Freedom becomes relative independence of immediate circumstances and ability to control them in the light of larger possibilities; it is neither dependent upon any mystical arbitrary force of will nor non-predictable, but is conditioned by degree of intelligence, by nutritional changes, and by training. This view, moreover, agrees with introspections as to our power to overrule immediate circumstances. We overrule them because they are inconsistent with the larger conditions that have shaped our behavior trends, both those that are innate and those that are acquired. Bodily stored energy becomes liberated in such a fashion as to inhibit incongruous activities and to bring about the completest expression of the inner determining drives, whether these are called purposes, instincts, wishes, or what not. In the words of Holt, the importance of the immediate stimulus recedes with the higher forms of behavior. Wrong conduct, he says, "is wrong simply because it is behavior that does not take into account consequences; it is not adjusted to *enough* of the environment; it will be made right by an enlargement of its scope and reach." [1] It is along this line that we must solve the problems of freedom and will in so far as they belong to psychology.

Certain currents of our American psychology have been too much dominated by the passive associationism which conceived the individual in infancy as a purely receptive being actuated wholly from without, a sort of *tabula rasa;* and much of the pseudo-psychology of the 'sub-conscious' is a revolt of man's unsophisticated nature against the outcome of such a view. There is no good reason in fact why

[1] Holt, E. B., 'The Freudian Wish,' p. 198.

scientific psychology should not emphasize the inner urge of the metabolic changes as important factors in instincts, and give association processes a lesser rôle in selection and reconstruction processes of our mental adjustments. Wundt, representing a continental European tendency, distinguished between 'passive combination' of mental processes, calling these association, and the more active type of combination, which he termed apperception. But this latter term does not explain anything so far as the selection in learning is concerned; neither does Stout's 'cumulative disposition,' stated in terms of meaning, nor the co-conscious of Prince, whether or not it be a reality. These conceptions all seem to be built on the view that consciousness is a causal agency, a sort of original force. McDougall's animism, though it seems to have been motivated by a real short-coming of our associationism in psychology, is subject to the same criticism.

Intelligence and learning are obviously closely related. In their higher aspects both involve the capacity to take in a large situation, and by response to remote conditions to delay action and inhibit motor tendencies aroused by immediate stimuli until they have been coördinated with the larger circumstances. It is thus comprehensible that an individual learns by erroneous responses and that errors may be rather quickly eliminated, incontrovertible facts which are enigmas to the frequency-recency view of learning. Learning processes, of course, differ materially among themselves and there are gradations in learning, ranging from the simplest contiguous associations, illustrated by mere passive adaptation to immediate conditions, to the more explicitly rational readjustment to situations involving ideational elements. In the latter case the organism must resist impulses to immediate response as these are checked by the more remote circumstances, and it therefore becomes subject to strains and stresses characteristic of deliberation. Many of these various learning processes have little relation to one another. If we speak of these modification possibilities as a general learning capacity, we can mean nothing definite at all. While in general there are close relationships among

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the higher forms of learning, the several capacities may occasionally vary greatly among different individuals, as when we find a college woman almost incapable of learning ball tossing, or seriously deficient in acquiring mathematical skill.

Intelligence refers more particularly to one's ability to be affected by a wide range of circumstances and to delay reaction to them while the significant elements are selected out and weighed with respect to their bearing on the attainment of any particular end. It is more closely related to the sort of adjustment found in the higher forms of ideational learning. That person is most intelligent who, with a given amount of experience and maturity, is most apt to perceive significant relations and to react discriminatively to them as distinct from the numerous irrelevant elements in the situations met. He is said to be far-sighted, sagacious, and so on, because he refrains from making immediate responses which entangle him in conflicts later on. Intelligence is not synonymous with mere quickness of learning; it implies in addition the constancy of purpose, the discriminative ability, and the persistency that keeps the desired end in view while means are varied till appropriate control of the circumstances are obtained. The readiness with which a given series of syllables may be fixed in mind is not necessarily a safe criterion of intelligence, for the latter term seems to refer more directly to one's inherited mechanism for *selective* responses of the sort mentioned above.

We need more studies comparing different persons of various learning rates in given performances as to their perseverance against obstacles under a uniform degree of encouragement, so that we may know more of the limits of learning. We may yet find that on a scale of increasing difficulty, fast learners toward more easily obtainable ends are not regularly able to go as far toward the mastery of complex and difficult problems as some slow learners. The relations of the rate of learning to limits of ability to learn are yet not well known, because our so-called physiological limits have usually been only the maximum speed obtainable,

or the point where gross errors easily measurable cease to occur, limits that leave us ignorant on many points of importance. Because of obvious practical difficulties involved in the getting of subjects for suitable experiments, we are still in great ignorance regarding limits of attainment in nearly all forms of learning. The information we have is based on so few cases as to leave any general conclusions as yet insecure.

Why this selective mechanism that we have called intelligence[1] is conscious in its operations we do not know. Science cannot look upon consciousness as an original force, interesting in itself and puzzling as it may be as an aspect of the processes of behavior. The problem of freedom, scientifically conceived, seems not to be a problem regarding any sort of spontaneous force, but rather one of how, by constantly enlarging integrations, the individual gets more and more relative independence of the influence of immediate stimuli and of the order in which they chance to come, and thereby becomes better oriented to the larger circumstances and the possibilities of further behavior which they offer.

[1] Professor Warren (*Human Psychology*, chs. 7, 18) suggests the use of intelligence 'as a broad term to cover all forms of individual adaptation, motor as well as intellectual.' My own use of 'intelligence' in this paper is similar to his in holding important the motor or response aspects of the individual, though this may not be sufficiently stressed; but I wish also to emphasize the ability or the tendency to respond to comprehensive and complex *situations* as against the response to immediate stimuli (the reconstruction of environment as well as the mere adaptation to it), also what we might call the *persistent occupation* with responses that are rather remote in their implication. 'Energy' does not express all that is intended in this last clause, but it is included. Such a view relates intelligence closely to learning, which certainly includes reconstructive aspects, and seems not to be incompatible with the current use of 'intelligence' as ascribed to animals; it makes important the real difference between man and animals. In the text of this paper entire justice may not be given to Pyle's view, in as much as he says: 'The general learning factor and the general intelligence factor are probably the same.' (*Op. cit.*, p. 172). I find it difficult, however, to think of these capacities as a 'factor.'

'Intellect' is a term that can conveniently be reserved to apply to or to designate in particular the more intellectual functions or aspects of an individual, those that Warren has emphasized in connection with this term, a meaning that is implied in saying of one, 'He has a good intellect, but is shiftless and neglectful of opportunities.' It usually accompanies good motor tendencies and general energy, but is occasionally very devoid of them.

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THE ORGANISMAL POINT OF VIEW IN THE STUDY OF MOTOR AND MENTAL LEARNING

BY J. A. MELROSE

Janesville, Wis.

The question as to whether human learning differs from animal learning in kind or only in degree has been difficult to answer clearly. Accordingly psychologists have differed in their views, with a tendency it seems for that view to gain adherents which holds that the difference is one of degree only. The writer hopes to set forth soon a view of the technic of 'thinking' based upon an analysis of language-use that shall attempt to show the unique factors in human learning.

In making this analysis of language habits we shall use the 'organismal' method as we have already applied it to the motor learning of animals.[1] The object of the present paper is to state in some detail the general theory and basis of the 'organismal' point of view in order that when we come to enter the difficult field of human psychology our problem will not be further confused by lack of clearness in the method employed. We shall therefore (1) discuss the 'organismal' view with reference to general psychological theory, (2) exemplify the 'organismal' method in the study of simple forms of motor learning, and (3) attempt to state the central problem of human psychology in the shaping which this 'organismal' method requires. It is hoped that the discussion will have some interest from the viewpoint of general philosophy of psychology as well as furnish us a helpful approach to a specific problem to be taken up at a later time.

It will not be amiss, I think, to have a word on the general philosophy of psychology since we are pleading the cause of a view quite fundamentally different at some points from the methods of both 'introspection' and 'behaviorism.' It

[1] PSYCHOL. REV., May, 1921.

is in any case wise procedure to examine frequently the general theory of a science, for while the general technic of the scientific method is everywhere the same, its proper application is contingent upon the nature of the facts to be examined and the character of the practical problems which the particular science raises. It is surely not saying too much to urge that one of the most fruitful sources of confusion in the organic sciences has been the uncritical carrying-over *in toto* of attitudes, aims, and modes of attack from sciences lower in the serial order without any serious attempt to adjust these to the requirements of known facts and problems of the science in question.

This uncritical attitude on major matters seems to me to have hindered psychology. It appears difficult for example for psychologists to get away from a certain pathos of profundity to ask just what psychology as a distinct concrete science is seeking. This seems to be due to an attitude carried over from the aims and practice of the inorganic sciences.

Just what is the problem of psychology anyway? When we come to apply to the study of psychology the discursive technic of science—its analysis and synthesis—what are we trying to do? Has the nature of the science any guidance for us? How far may we abstract within this science if we are to be strictly scientific and how far may we generalize? Now there are certain very obvious differences in our datum and our problem from those of the inorganic sciences. Do these differences lay down for us the conditions of successful method? There seems to be a quite general feeling in psychology that we are especially seeking something very *fundamental* in the biologic deeps—that we are after something very *primary* in the 'psychic' or physiological sense—just as the inorganic sciences are seeking something very fundamental in the organization of matter. But are we?

"Science is for prevision." Prevision depends upon the nature of the datum and the shaping of the problem. In saying this I am not of course urging merely the so-called

'practical' claims of science unmindful of what we owe to pure science. But no science neglects pragmatic matters. Whether our problem is in 'pure' or 'practical' science[1] it is equally important that we make our attack with the *real nature of our datum and problem in mind* in order that our abstracting shall not be *arbitrary but observational*, and our generalizations shall not be *superimposed upon the facts but discovered in them.*

Now the sciences of physics and chemistry are seeking something which is very fundamental. They are after that which is primary in the organization of matter itself, for their task is to break matter up and refabricate it. This being the case the more primary the discovery the more far-reaching the conclusions. Moreover the structure of inorganic matter seems to lend itself to be broken up in a more and more primary way. Hence fundamental discoveries like that of radio-activity are very far-reaching and have the greatest achieving value in the theory and practice of the science.

Similarly psychology appears to conclude that the most important discoveries it could make would be those most fundamental in the *biologic* sense. So we have been trying to find the nature of mind, the technic of primary neural organization, or the secrets of muscular and glandular behavior—*always some profound fact in the depths of the organism itself.* Granting always the undoubted importance of any careful research in so fertile a field, may we not ask if this is not a turned-about, mistaken emphasis—the projection of false profundity which really hurts progress in the long run? Is there any good reason in the nature of our datum and problem for believing that the discovery of the most primary life-facts connected with learning would help most in solving the really pragmatic problems of psychology? Is there any reason to believe that the discovery of facts very far from primary in the 'psychic' and physiological sense may not prove to be the most dynamic discoveries for the science?

[1] The terms 'pure' and 'practical' are merely a convenience. All science is pure. So-called practical science is simply closer to concrete application.

In short does the general method of breaking up into elemental parts and reshaping—the method of the inorganic sciences—hold in psychology? Does the well-known nature of the facts and problems of psychology favor it? Is not almost the contrary rather the case?

So unconscious ofttimes are our preconceptions and so important our general orientation to a problem or set of problems, that I may perhaps be pardoned some repetition in pressing a point of view somewhat different from that which I understand to be customary. We said that the problem of the physicist for example is to manipulate matter as far as possible and isolate it part from part, and the further he can push his analysis the more far-reaching his results when he refabricates it. But does anyone suppose that the situation is similar in the organic sciences and especially in psychology? Surely we do not assume that even if we could analyze out some profound biologic secret connected with learning we could take a living organism part from part and put it together again or otherwise fabricate its structure. Now if we may not hope to manipulate parts and processes directly within the organism, the field of manipulation is a limited one. We would have to manipulate indirectly by means of outside factors and this would alter the shaping of the whole problem.

If we cannot break organisms down and build them up again thereby changing learning processes, our field of control is limited to manipulating such factors as media, food, chemicals, temperature, location and quality of stimuli, etc.—factors outside of the organism. This appears to me to be the case. The problem of the 'individualism of the organism' extends over the whole field of biology and is, despite much study, in a state of complete flux. It would seem wise for psychology to frankly assume this individualism and go straight to the problem of how the organism *as a unit* makes its adaptations rather than either attack the general problem of the nature of the integrity of the organism or what is worse *attempt an elemental analysis which ignores it.*

Now if we are to accept the organism and the obvious

fact that it makes its adaptations as an integrated whole[1] such a view shifts the main emphasis outside of the organism to the different *types of learning* or adaptation which animals achieve, and the relative importance of these types with reference to particular problems. If we cannot hope to change the processes of learning themselves, it is our task to apply selective intelligence to knowing the different *types or patterns of adaptation, their relative merit for particular problems,* and the *conditions favorable to the controllable factors upon which the respective types are built up.*[2]

This view simplifies the problem and radically changes the method. It does not exclude any other problems but merely brings a different problem to the fore *which a more pragmatic emphasis in theory makes primary.* In attacking our problem as we have stated it—a problem simple and objective,—we may assume for the present a great deal under the unity of the organism and any problems in our assumptions come later in serial order and can wait. Besides whatever problems are neglected for the time and whatever their relevancy for psychology they are not primary to this science and they appear to be such for the most part as will be more open to solution after we have dealt successfully with the more objective problem concerning the *types* of

[1] When a reaction is being learned there is a marked 'set' of the organism, even very slight movements are accompanied by this noticeable 'set' of the whole. Well-learned reactions show a minimum of general 'set' or 'attention.' The same is true of 'thinking' as of motor reaction.

[2] Does anyone doubt the tremendous dynamic value for educational theory there would be in a well-organized body of facts on the *types* of learning of which organisms are capable, the relative merit of different *types* for different kinds of problems, the factors that shape the respective *types* and that control their accuracy and rapidity in function, and the factors that facilitate substitution and integration of *types*? Are we so sure about other matters? Suppose we could come into possession of a like body of facts concerning the primary physiological structure and activity associated with the adaptation of organisms. Is it clear that this information would be of large pragmatic value for psychology? Might we not still be limited to the same controllable factors as before? To be sure such discoveries, assuming them within reach at the present time, might throw new light upon controllable factors or even open up new and unseen possibilities. We have no right to assume that they would not. What we are urging is this. The first body of facts appears to be within reach of research now and is of undoubted pragmatic value; the second raises questions both as to its possibility and value.

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learning by which organisms actually adapt to their environment, and have noted the controllable factors from which the types have been built up. Be that as it may *the order we are proposing in the attack seems to be based upon a serial order in the problem itself.*

The behaviorist appears to have seen this need of getting at the more pragmatic facts of learning. But in emphasizing actual observation of overt behavior he was unfortunate in *defining his datum.* He was too precipitous and crystalized his theory too soon. He does not appear to have seen sharply the *distinctive* problem of psychology, although his mind was moving in the right direction. The abstracting tendency led him astray. Instead of taking the behavior of the *organism as a whole* as his datum and passing from this integrated whole to a study of the behavior of parts also, he studied the reactions of 'muscles and glands' and attempted to arrive at conclusions for the whole organism by piecing together results.[1]

This it appears is sure to be an unsound method.[2] It might seem at first thought to be a possible method in the study of very simple organisms, but when you come up to higher levels its weakness is apparent. With respect to 'thinking,' the very highest learning, even the data themselves which are to be observed by the behavioristic theory, become an *ignis fatuus* which almost every one admits to exist and which no one can reach and evaluate. When we are on the lower levels the emphasis of actual observation is often 'organismal' and the behaviorist seems therefore to be starting us out upon a scientific trail broad and clear, but as we come up to higher levels, where abstracting out the reactions of 'muscles and glands' becomes prominent, the trail disappears into a squirrel-hole leaving nothing for the

[1] It is interesting to note that the behaviorist position under fire accepts the wholeness of the reaction. Watson says in a rejoinder in the *British Journal of Psychology*, October, 1920: "A man thinks with his whole body in each and every part."

[2] Much of the behaviorist's work is fruitful because it is better than his theory. A good deal of his *observation* is free from the false abstracting of his *doctrine.*

behaviorist to do but impotently tell us *what we would find if only someone could pursue the way and find it.*[1]

The writer believes all this trouble comes from an uncritical abstracting tendency. It is born of a failure to see sharply the distinctive problem of psychology as contrasted with the problems of the inorganic sciences. The inorganic sciences are after elemental parts. Psychology does not neglect these, but it does well to remember perhaps that the integrity of the organism is its most primary fact. Psychology should study therefore first of all the behavior of the organism treated as a unit and when the parts are studied they should be studied as parts. This distinction is always important but is especially necessary in practice on the higher levels. The organism is accepted as a whole simply because for the present at least *we cannot get behind it.* So long as we cannot take the various elements of the body of a living organism apart and put them together again *we cannot legitimately abstract them out and think finally to get results for the whole organism by a summation of separate results.*

An organism is a whole. This is our first fact. What we first want to know therefore as students of psychology are the *patterns* of the adaptations which the *organism as a whole* makes to its environment and the *controllable factors* which so affect the environment and the organism as to modify learning. If as we have said an attack upon this problem invites a study of the behavior of the elemental parts of an organism, as well it may in many cases, the part must be approached with reference to the whole—*the main datum*—and not the whole with reference to the part.

This position which urges us to accept the wholeness of the organism in our assumptions appears to be sound. This wholeness is the most distinctive fact which separates the organic from the inorganic sciences. Paton reminds us[2] that

[1] Watson says that if we could bring out 'thinking' for observation as you can rowing and tennis playing, the need for explaining it would disappear.

[2] 'Human Behavior,' '21, p. 28. Paton emphasizes in general the same approach we are stressing and I am indebted to him for the name 'organismal' as applied to it although I had applied the method in detail in my former papers before reading his book. It is not clear to me how he would apply the method to concrete problems.

the distinction between living and inorganic matter is not so much in the specificity of any particular reaction as in the totality and integrity of the reactions which are peculiar to living organisms. Surely then the most unarbitrary and consistent move which psychology can make is to accept for the present the organism as a whole and,—rejecting all pathos of profundity, psychic or physiological—attempt first the commonplace task of isolating the patterns of the different types of learning which enter into the actual adaptations of organisms from the lower to the higher levels, and evaluating the controllable factors which enter into the building of the various types. This appears to be for the present not only the most important function of psychology as a concrete science, but it also looks like the serial step *most likely to open the road to deeper facts of physiological structure which condition the learning process.*

We of course are not urging that the study of elemental parts of an organism is unprofitable for psychology. Far from it. We however urge that since psychology has to do with the laws of learning and since organisms as integrated wholes make adaptations to definite problems and thereby make progress in learning,—that therefore the first interest of psychology should be to isolate the laws (patterns, types) of these adaptations and the factors which shape them, and evaluate these types and factors *in order to control and advance learning.* This would seem to be more important for psychology as a primary step than to inquire into the nature of the organism. Besides it is by no means certain that we shall not always be able to judge better concerning the fundamental bodily structure and activity which is concomitant to higher adaptive adjustments by a study of the *patterns of the overt behavior* than we ever shall be from a direct examination of *living tissue.* This may always be the only open road. At least there seems to be little doubt as to *where the emphasis should rest at the present stage of progress.*

Accordingly we attempted to set forth in the PSYCHOLOGICAL REVIEW, May, 1921, the types of learning which

enter into the learning of sub-human animals.[1] Also the March, 1922, number of the PSYCHOLOGICAL REVIEW has a paper which attempts from the same 'organismal' point of view to interpret the dilemma between the 'introspective' and 'behavioristic' attack upon the problem of 'thinking.'[2] We found in the latter paper that when we come to the problem of human learning, 'introspection' lacks a fully *objective method* and 'behaviorism' lacks an *objective datum*, and we offered some reasons to show that an analysis of language habits from the organismal point of view should furnish both an *observational datum and an objective method.*

It is our purpose now to exemplify the 'organismal' method upon simple examples of motor learning, having as our especial aim to set forth the theory upon which this method is based in its detailed application to the problem of learning. This may be done by isolating the two lowest types of learning which organisms employ after the manner used in the former paper on animal learning,[3] and calling attention to the technic of the method by which the types are isolated, and the inferences which we are able to draw concerning concomitant physiological structure and activity from the structure of the pattern which learned behavior takes. When we have done this we shall be ready to set up the *distinctive problem of human psychology—the problem of 'thinking', in terms of the 'organismal' theory and method.*

Let us suppose we were observing the earliest movements of a just-hatched fish embryo. We note that it makes now and then a struggling wabbling movement in which it turns up somewhat from the dorsal toward the ventral side and moves along a short distance in the general direction of its main axis. As the effort ceases the embryo turns back each time to the dorsal position and lies quiet for a short period. Now we observe two things as we watch the re-

[1] 'The Structure of Animal Learning.'

[2] 'The Crux of the Psychological Problem.'

[3] The two types chosen, 'organic fixation,' and 'organic spacial adaptation' are typical. The observations which we shall make apply equally to the other types which animals use—'organic choice,' 'organic association,' 'organic conception' and 'organic judgment.'

peated 'trials.' First, we note that the organism is gaining both in reaching more nearly the ventral position and in moving faster and farther. Again, we notice that the direction of the movement, at first that of a spiral, is slowly straightening out toward a right line. This process keeps up until the individual has achieved the normal swimming habits of the adult fish, in which it lies on its ventral side and swims mainly in a straight course.

Now it is apparent that if we observed many individuals pass through this process we would conclude—unless we were to say dogmatically that the whole process is one of maturation only—that here is a general type of learning. We would therefore ask what is the fundamental nature or pattern of this type,—that which is peculiar to it and always present in it?

So primitive and simple is the behavior here that the question is easily answered. A wabbling incoördinate movement has become fixed and controlled. It is apparent also that since the medium in which the embryo swims remains the same, the change which takes place in the reaction is guided from the structure of the organism itself. We conclude therefore that the effects of the repeated movements help to tighten up the inherited bilateral structure of the organism. We shall call this *type* (*1*). The fundamental thing about this type is that it is the *fixing of a reaction with reference to structure in the organism itself.*

Before we draw any further conclusions with reference to this type of learning let us turn to another type that sometimes looks from a superficial point of view very similar. We shall give it first in a form where the overt behavior looks quite different and then exemplify it so that the superficial spacial behavior is similar to the type we have just noted.

Suppose any one of the vermes to be placed in a narrow alley and every time it approaches the left side of the alley it is pricked sharply with a needle-point. If the same individual is put many times in the alley always facing the same way and subjected to the treatment noted above, it will be ob-

served that he finally *ceases to turn to the left* but holds closely to the right side of the alley. But suppose rather that we both prick the animal on the left side when he approaches that side and on the right side when that side nears the side of the alley, what then? In this case the organism becomes cautious as it were in his movements and by repeated experiments learns to avoid turning far in either direction but *follows a fairly straight course* down the middle of the alley. We shall call the learning of this kind *type* (2).

Now in the first part of this experiment the animal learns to avoid turns to the left but still turns freely to the right. The spacial pattern in this case does not resemble that of fixation noted in type (1). But in the second instance the two patterns of types (1) and (2) are similar spacially. They both represent a zigzag course straightening out toward a right line. But in type (2) the learning is not due primarily to the nature of the animal's structure as is the former type. The stimulations which produced the learning came from the outer environment, and the type of the reaction clearly shows that it is built up on the basis of the location of these points of stimulation and the movement of the animal. This represents *organic space adaptation* in simple pattern.

We have now two distinct types of learning—an organism adjusting movement to its own inherited structure by practice and an organism adjusting its movement to the structure of its environment by practice. We have called them 'organic fixation' and 'organic space adaptation.' They represent adjustment to two distinct problems of the organism. These two types of learning do not appear to be correlated with differences in physiological processes but to differences in the *structure of the adaptive problem.* The end of the adaptive reaction is in each case the equilibrium of the organism in movement *with reference to this structure of the problem.*

It is clear from our observations above that we can see the true *pattern* of a type of learning only after we have found the *constant* elements in the behavior. To find these

constant elements, which are the structure of a pattern of learning, we would observe many examples of the behavior of each general type whose structure we are seeking. Having observed any general type of behavior repeatedly so as to eliminate the effects of chance and ephemeral factors, we next observe the relation of the constant elements in the *learning reactions* to the constant elements in the *structure of the environmental problem.*

For example in type (1) we would take care that many observations were made and that the medium was kept the same throughout the learning. Several specimens would be tried to eliminate the effects of variable elements in the individuals. Since the learning in the several cases *follows a constant pattern* we conclude that *control factors* are at work. We seek them in the situation. Since in type (1) there are none in the outward situation which can account for the learning we judge that the pattern is correlated with control elements in the *structure of the organism itself.* The nature of the pattern of the learning leads us to believe that it is the product of the *bilateral structure of the organism* and some *physiological process within the organism which assimilates the effects of the various movements.*

Also with respect to type (2) we follow the same method of observation, analysis and conclusion. It is a well-known fact that worms do not always turn to the right nor do they follow normally a straight course. The specimens observed do not at first. We conclude that the constants in this learning are therefore not due to the normal structure of the organisms tested. We note on the contrary that the structure of the learning pattern bears an obvious relation to the *structure of the environment* in which the learning was repeatedly run off. The learned behavior is clearly and definitely correlated with the location spacially of the points of stimulation. We judge then that this type of learning is accounted for by the *loci of the stimuli* with reference to the moving organism, together with the same *physiological process which assimilates the effects of the various movements.*

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Let us now look at the two types together. While they represent two distinct types of reaction to two distinct types of problems they both appear to root back in the *same physiological process* which is at the base of learning. In each case there are the *effects of movement, the assimilation or synthesis of effects*, and *the projection of the final result* in the learned reaction. The differences in the two types do not appear to be accounted for at all by differences in *physiological processes* but solely by differences in the *structure of the adaptive problem.*

These two types are typical. It will be found that the generalizations which we have arrived at are equally applicable to the other types of learning of which animals below man are capable.[1] Let us state these generalizations in brief form:

1. Patterns of learning appear to be correlated not with differences in physiological processes but with *differences in the structure of the problem of adaptation.*

2. We isolate the final pattern of a type of learning in reference to the relation existing between *constants* (structure) *in the learning behavior* to *constants* (structure) *in the adaptive problem.*

3. Patterns are explained from the effects of constants (structure) in the *environmental situation* or constants in the *bodily organization of the animal.* When learning patterns are not accounted for by the structure of the environment they cannot be deemed due to chance but must be due to *control structure in the organism itself.*

It follows, if the above are sound, that learning roots in a fundamental life-fact,—a secret in the depths of the organism. But we must note that this mysterious fact is *present in all learning*—in the very lowest as well as in higher types. It cannot therefore be appealed to to explain progress from type to type. But this *progress* is what is of supreme interest. The fact of learning would be of slight interest if it remained at the lowest levels. The important emphasis rests therefore on those factors which, given the physiological

[1] 'The Structure of Animal Learning,' PSYCHOL. REV., May, 1921.

process at the base of learning, *account for the shaping of the respective types of learning from the lower to the very highest levels.*

We have just pointed out that these types represent constant relations between (1) the shaping of learned behavior and (2) the structure of the organism and the environment—that is, they are shaped in relation to the *life-needs* of the organism and the structure of *life-situations.* The effects upon learning which come from the structure of the organism itself are of course important for certain problems, but those from the structure of the environment appear to be even more important in actually shaping learning patterns. Given the general structure of the organism, the *structure of the environmental problem* determines the pattern of the learning which adapts to it. It is on this account that *the pattern of the behavior of a man in a maze is like that of the rat in the maze*, just as Watson says. It could not be otherwise, for the *environmental problem shapes the end-result.* It does not therefore prove anything with reference to 'thinking' as Watson appears to conclude. Proof with reference to the value of 'thinking' must come by a quite different route.

Let us see. Since adaptive patterns have reference to the structure that shapes them, if we would know the value of human learning we should seek the *pattern of distinctively human behavior.* If any motor problem whatever must in the nature of the case require from any individual solving it, whether animal or man, the *same pattern*, simply because the *structure of the problem shapes the pattern*, the way to test man for distinctive learning is to observe him in adaptations of which he alone is capable and look for the pattern of the behavior. Or again, if the pattern of learning—the end-result—is fixed from the effects of either (1) the constants in the environment in which the learning is built up (2) or constants in the inherited technic of the organism, the way to get at the human learning is to look for the *pattern of the most characteristically human behavior* and try

to evaluate the pattern. Such behavior is obviously the *use of language as the overt expression of 'thinking.'*

The problem then becomes one of observing language-use, in order to discover if possible in this mode of behavior the relations which may exist between (1) *language habits* and (2) the constants in the situation out of which language-use is built up or constants in the inherited structure of man himself as a separate species. But the 'organismal' method with its eye on the adaptive problem leaves aside, as off the main trail, the 'muscular and glandular' reactions associated with language-use, and keeps in mind the use of language as related to *the function of adaptation.* From this point of view language is referred to the *ear. It is a sign-system heard.*[1] As such we observe it as the overt expression of 'thinking' and look for its fundamental *pattern.*

Let us now turn back to the three generalizations which we drew from our observation of the two types of primitive learning, and which we asserted were equally applicable to all learning of sub-human animals, and view our problem of human learning in the setting that this exemplification of the 'organismal' method in animal learning gives us. We shall merely set our problem within the rules as a way of stating the problem. That is, the generalizations are discursive and we shall fit the terms of our problem into them and offer them in this form as a definite statement of the *problem of 'thinking' by the 'organismal' method.*

1. The significance of 'thinking' is not to be sought in some different biologic process peculiar to it but in any *distinctive structure which may be found in language habits.*

2. The pattern (if any) of thinking is to be sought in its overt behavior in language-use, by seeking the relation between constants (*a*) in *language habits* and constants (*b*) in the *environmental situations* in which these habits are built up or in *inherited bodily structure* employed in language use.

3. Any patterns in language habits must be due to either (*a*) constants in the *environmental situations* in which language

[1] 'The Crux of the Psychological Problem,' PSYCHOL. REV., March, 1922.

grew up or (*b*) constants in the *inherited mechanism* of 'thinking' of which language is the overt expression. Any constant pattern in language-use which is not accounted for by constants in the environmental situations in which language habits have been built up cannot be deemed due to chance but must be due to the effect of *control elements in man's inherited structure.*

Finally our whole problem may be generalized as follows. If upon examination there should be found no pattern in the use of language—the overt expression of 'thinking'—which is not accounted for by (1) the structure of the environmental problems of language and (2) lower forms of learning, then it would appear that there is nothing distinctive in man's inherited learning technic. If however *a definite constant pattern is found in the behavior of language-use which remains unaccounted for by* (1) *lower types of learning or* (2) *the constants in the environmental situations in which language habits are formed, then this pattern must be deemed due to man's inherited structure. Moreover this constant pattern would be definitely correlated with this inherited structure and would represent the structural difference between 'thinking' and animal learning (motor learning).*

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DISCUSSION

A FRIENDLY CHALLENGE TO PROFESSOR THORNDIKE

If ever anyone was fortunate in his scientific adversary, I deem it to be myself when having to oppose Professor Thorndike. Each of his controversial writings brings anew the pleasure of his courteous address, the instruction from his invariably valuable communications, and not least the gratification of finding that after all the difference between us really lies in some superficial misunderstanding.

All this has once more happened in the case of his recent brief but pregnant paper "On the Organization of the Intellect."[1] His main thesis is that the correlation between the columns in a table of correlations has been found by him to average, in one instance .35, and in another .46; whereas my theory, he says, demands an approximation to unity.

Unfortunately, however (waiving other objections to his argument), the said intercolumnar correlation used by him is quite inapplicable to his particular cases. Its employment is in conflict with the fundamental limitation "that in each of these columns the mean square deviation should be at least double the correction to be applied to that deviation."[2] This inter-columnar correlation had been put forward by me as an approximate criterion usable solely under the above limitation. My exact and *general* criterion was the equation $r_{ap}/r_{aq} = r_{bp}/r_{bq}$, where a, b, p, and q are any of the variables.[3] But for this criterion I had not worked out the needful probable error and correction. I hope to do so in a few days, and will then apply it to his new data, publishing the results.

The above I regard as only a slip. But in another respect I do complain of a genuine grievance. Thorndike and others treat, and even expressly speak of, the "rigid unqualified form of Spearman's doctrine."[4] But why, in the name of goodness, *should* it be taken as 'rigid' and 'unqualified'? There exists a third course besides either flatly rejecting a theory or accepting it without qualification; that is, helping to develop it. My own very first

[1] PSYCHOL. REV., **28**, 1921, p. 141.

[2] *Brit. J. Psychol.*, **5**, 1912, p. 56.

[3] *Ibidem*, p. 58.

[4] His paper, p. 149.

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enunciation of this theory explicitly spoke of "its inevitable eventual corrections and limitations."[1] At these I have been myself working for eighteen years, and propose to give some account of them when expounding the theory in more detail (this year or next).

In mean time, to save further uneconomical misunderstandings, I hereby venture the following challenge. Let Thorndike and myself agree as to what (if any?) ascertainable fact would be in harmony with the view of the one of us and not the other. Let us agree upon and carry out the same procedure of investigation. And then let us produce and explain our respective results side by side. If a third and impartial investigator can be found, so much the better.

C. SPEARMAN

UNIVERSITY COLLEGE, LONDON

[1] *Amer. J. Psych.*, XV., 1904, p. 273.